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THE VALUE OF SCIENCE[1]

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Introduction

THE search for truth should be the goal of our activities; it is the sole end worthy of them. Doubtless we should first bend our efforts to assuage human suffering, but why? Not to suffer is a negative ideal more surely attained by the annihilation of the world. If we wish more and more to free man from material cares, it is that he may be able to employ the liberty obtained in the study and contemplation of truth.

But sometimes truth frightens us. And in fact we know that it is sometimes deceptive, that it is a phantom never showing itself for a moment except to ceaselessly flee, that it must be pursued further and ever further without ever being attained. Yet to work one must stop, as some Greek, Aristotle or another, has said. We also know how cruel the truth often is, and we wonder whether illusion is not more consoling, yea, even more bracing, for illusion it is which gives confidence. When it shall have vanished, will hope remain and shall we have the courage to achieve? Thus would not the horse harnessed to his treadmill refuse to go, were his eyes not bandaged? And then to seek truth it is necessary to be independent, wholly independent. If on the contrary we wish to act, to be strong, we should be united. This is why many of us fear truth; we consider it a cause of weakness. Yet truth should not be feared, for it alone is beautiful.

When I speak here of truth, assuredly I refer first to scientific

[1] Authorized translation by Professor George Bruce Halsted, Ph.D. Copyright, 1906, by The Science Press.

truth; but I also mean moral truth, of which what we call justice is only one aspect. It may seem that I am misusing words, that I combine thus under the same name two things having nothing in common; that scientific truth, which is demonstrated, can in no way be likened to moral truth, which is felt. And yet I can not separate them, and whosoever loves the one can not help loving the other. To find the one, as well as to find the other, it is necessary to free the soul completely from prejudice and from passion; it is necessary to attain absolute sincerity. These two sorts of truth when discovered give the same joy; each when perceived beams with the same splendor, so that we must see it or close our eyes. Lastly, both attract us and flee from us; they are never fixed: when we think to have reached them, we find that we have still to advance, and who pursues them is condemned never to know repose. It must be added that those who fear the one will also fear the other; for they are the ones who in everything are concerned above all with consequences. In a word, I liken the two truths, because the same reasons make us love them and because the same reasons make us fear them.

If we ought not to fear moral truth, still less should we dread scientific truth. In the first place it can not conflict with ethics. Ethics and science have their own domains, which touch but do not interpenetrate. The one shows us to what goal we should aspire, the other, given the goal, teaches us how to attain it. So they can never conflict since they can never meet. There can no more be immoral science than there can be scientific morals.

But if science is feared, it is above all because it can not give us happiness. Of course it can not. We may even ask whether the beast does not suffer less than man. But can we regret that earthly paradise where man brute-like was really immortal in knowing not that he must die? When we have tasted the apple, no suffering can make us forget its savor. We always come back to it. Could it be otherwise? As well ask if one who has seen and is blind will not long for the light. Man, then, can not be happy through science, but to-day he can much less be happy without it.

But if truth be the sole aim worth pursuing, may we hope to attain it? It may well be doubted. Readers of my little book 'Science and Hypothesis' already know what I think about the question. The truth we are permitted to glimpse is not altogether what most men call by that name. Does this mean that our most legitimate, most imperative aspiration is at the same time the most vain? Or can we, despite all, approach truth on some side? This it is which must be investigated.

In the first place, what instrument have we at our disposal for this conquest? Is not human intelligence, more specifically the intelligence

of the scientist, susceptible of infinite variation? Volumes could be written without exhausting this subject; I, in a few brief pages, have only touched it lightly. That the geometer's mind is not like the physicist's or the naturalist's, all the world would agree; but mathematicians themselves do not resemble each other; some recognize only implacable logic, others appeal to intuition and see in it the only source of discovery. And this would be a reason for distrust. To minds so unlike can the mathematical theorems themselves appear in the same light? Truth which is not the same for all, is it truth? But looking at things more closely, we see how these very different workers collaborate in a common task which could not be achieved without their cooperation. And that already reassures us.

Next must be examined the frames in which nature seems enclosed and which are called time and space. In 'Science and Hypothesis' I have already shown how relative their value is; it is not nature which imposes them upon us, it is we who impose them upon nature because we find them convenient. But I have spoken of scarcely more than space, and particularly quantitative space, so to say, that is of the mathematical relations whose aggregate constitutes geometry. I should have shown that it is the same with time as with space and still the same with 'qualitative space'; in particular, I should have investigated why we attribute three dimensions to space. I may be pardoned then for taking up again these important questions.

Is mathematical analysis then, whose principal object is the study of these empty frames, only a vain play of the mind? It can give to the physicist only a convenient language; is this not a mediocre service, which, strictly speaking, could be done without; and even is it not to be feared that this artificial language may be a veil interposed between reality and the eye of the physicist? Far from it; without this language most of the intimate analogies of things would have remained forever unknown to us; and we should forever have been ignorant of the internal harmony of the world, which is, we shall see, the only true objective reality.

The best expression of this harmony is law. Law is one of the most recent conquests of the human mind; there still are people who live in the presence of a perpetual miracle and are not astonished at it. On the contrary, we it is who should be astonished at nature's regularity. Men demand of their gods to prove their existence by miracles; but the eternal marvel is that there are not miracles without cease. The world is divine because it is a harmony. If it were ruled by caprice, what could prove to us it was not ruled by chance?

This conquest of law we owe to astronomy, and just this makes the grandeur of the science rather than the material grandeur of the objects it considers. It was altogether natural then that celestial

mechanics should be the first model of mathematical physics; but since then this science has developed; it is still developing, even rapidly developing. And it is already necessary to modify in certain points the scheme I outlined in 1900 and from which I drew two chapters of 'Science and Hypothesis.' In an address at the St. Louis exposition in 1904, I sought to survey the road traveled; the result of this investigation the reader shall see farther on.

The progress of science has seemed to imperil the best established principles, those even which were regarded as fundamental. Yet nothing shows they will not be saved; and if this comes about only imperfectly, they will still subsist even though they are modified. The advance of science is not comparable to the changes of a city, where old edifices are pitilessly torn down to give place to new, but to the continuous evolution of zoologic types which develop ceaselessly and end by becoming unrecognizable to the common sight, but where an expert eye finds always traces of the prior work of the centuries past. One must not think then that the old-fashioned theories have been sterile and vain.

Were we to stop there, we should find in these pages some reasons for confidence in the value of science, but many more for distrusting it; an impression of doubt would remain; it is needful now to set things to rights.

Some people have exaggerated the rôle of convention in science; they have even gone so far as to say that law, that scientific fact itself, was created by the scientist. This is going much too far in the direction of nominalism. No, scientific laws are not artificial creations; we have no reason to regard them as accidental, though it be impossible to prove they are not.

Does the harmony the human intelligence thinks it discovers in nature exist outside of this intelligence? No, beyond doubt, a reality completely independent of the mind which conceives it, sees or feels it, is an impossibility. A world as exterior as that, even if it existed, would for us be forever inaccessible. But what we call objective reality is, in the last analysis, what is common to many thinking beings, and could be common to all; this common part, we shall see, can only be the harmony expressed by mathematical laws. It is this harmony then which is the sole objective reality, the only truth we can attain; and when I add that the universal harmony of the world is the source of all beauty, it will be understood what price we should attach to the slow and difficult progress which little by little enables us to know it better.

Intuition and Logic in Mathematics

I.

It is impossible to study the works of the great mathematicians, or even those of the lesser, without noticing and distinguishing two opposite tendencies, or rather two entirely different kinds of minds. The one sort are above all preoccupied with logic; to read their works, one is tempted to believe they have advanced only step by step, after the manner of a Vauban who pushes on his trenches against the place besieged, leaving nothing to chance. The other sort are guided by intuition and at the first stroke make quick but sometimes precarious conquests, like bold cavalrymen of the advance guard.

The method is not imposed by the matter treated. Though one often says of the first that they are *analysts* and calls the others *geometers*, that does not prevent the one sort from remaining analysts even when they work at geometry, while the others are still geometers even when they occupy themselves with pure analysis. It is the very nature of their mind which makes them logicians or intuitionalists, and they can not lay it aside when they approach a new subject.

Nor is it education which has developed in them one of the two tendencies and stifled the other. The mathematician is born, not made, and it seems he is born a geometer or an analyst. I should like to cite examples and there are surely plenty; but to accentuate the contrast I shall begin with an extreme example, taking the liberty of seeking it in two living mathematicians.

M. Méray wants to prove that a binomial equation always has a root, or, in ordinary words, that an angle may always be subdivided. If there is any truth that we think we know by direct intuition, it is this. Who could doubt that an angle may always be divided into any number of equal parts? M. Méray does not look at it that way; in his eyes this proposition is not at all evident and to prove it he needs several pages.

On the other hand, look at Professor Klein: he is studying one of the most abstract questions of the theory of functions to determine whether on a given Riemann surface there always exists a function admitting of given singularities. What does the celebrated German geometer do? He replaces his Riemann surface by a metallic surface whose electric conductivity varies according to certain laws. He connects two of its points with the two poles of a battery. The current, says he, must pass, and the distribution of this current on the surface will define a function whose singularities will be precisely those called for by the enunciation.

Doubtless Professor Klein well knows he has given here only a

sketch: nevertheless he has not hesitated to publish it; and he would probably believe he finds in it, if not a rigorous demonstration, at least a kind of moral certainty. A logician would have rejected with horror such a conception, or rather he would not have had to reject it, because in his mind it would never have originated.

Again, permit me to compare two men, the honor of French science, who have recently been taken from us, but who both entered long ago into immortality. I speak of M. Bertrand and M. Hermite. They were scholars of the same school at the same time; they had the same education, were under the same influences; and yet what a difference! Not only does it blaze forth in their writings; it is in their teaching, in their way of speaking, in their very look. In the memory of all their pupils these two faces are stamped in deathless lines; for all who have had the pleasure of following their teaching, this remembrance is still fresh; it is easy for us to evoke it.

While speaking, M. Bertrand is always in motion; now he seems in combat with some outside enemy, now he outlines with a gesture of the hand the figures he studies. Plainly he sees and he is eager to paint, this is why he calls gesture to his aid. With M. Hermite, it is just the opposite; his eyes seem to shun contact with the world; it is not without, it is within he seeks the vision of truth.

Among the German geometers of this century, two names above all are illustrious, those of the two scientists who have founded the general theory of functions, Weierstrass and Riemann. Weierstrass leads everything back to the consideration of series and their analytic transformations; to express it better, he reduces analysis to a sort of prolongation of arithmetic; you may turn through all his books without finding a figure. Riemann, on the contrary, at once calls geometry to his aid; each of his conceptions is an image that no one can forget, once he has caught its meaning.

More recently, Lie was an intuitionalist; this might have been doubted in reading his books, no one could doubt it after talking with him; you saw at once that he thought in pictures. Madame Kovalevski was a logician.

Among our students we notice the same differences; some prefer to treat their problems 'by analysis,' others 'by geometry.' The first are incapable of 'seeing in space,' the others are quickly tired of long calculations and become perplexed.

The two sorts of minds are equally necessary for the progress of science; both the logicians and the intuitionalists have achieved great things that others could not have done. Who would venture to say whether he preferred that Weierstrass had never written or that there had never been a Riemann? Analysis and synthesis have then both

their legitimate rôles. But it is interesting to study more closely in the history of science the part which belongs to each.

II.

Strange! If we read over the works of the ancients we are tempted to class them all among the intuitionalists. And yet nature is always the same; it is hardly probable that it has begun in this century to create minds devoted to logic. If we could put ourselves into the flow of ideas which reigned in their time, we should recognize that many of the old geometers were in tendency analysts. Euclid, for example, erected a scientific structure wherein his contemporaries could find no fault. In this vast construction, of which each piece however is due to intuition, we may still to-day, without much effort, recognize the work of a logician.

It is not minds that have changed, it is ideas; the intuitional minds have remained the same; but their readers have required of them greater concessions.

What is the cause of this evolution? It is not hard to find. Intuition can not give us rigor, nor even certainty; this has been recognized more and more. Let us cite some examples. We know there exist continuous functions lacking derivatives. Nothing is more shocking to intuition than this proposition which is imposed upon us by logic. Our fathers would not have failed to say: "It is evident that every continuous function has a derivative, since every curve has a tangent."

How can intuition deceive us on this point? It is because when we seek to imagine a curve, we can not represent it to ourselves without width; just so, when we represent to ourselves a straight line, we see it under the form of a rectilinear band of a certain breadth. We well know these lines have no width; we try to imagine them narrower and narrower and thus to approach the limit; so we do in a certain measure, but we shall never attain this limit. And then it is clear we can always picture these two narrow bands, one straight, one curved, in a position such that they encroach slightly one upon the other without crossing. We shall thus be led, unless warned by a rigorous analysis, to conclude that a curve always has a tangent.

I shall take as second example Dirichlet's principle on which rest so many theorems of mathematical physics; to-day we establish it by reasonings very rigorous but very long; heretofore, on the contrary, we were content with a very summary proof. A certain integral depending on an arbitrary function can never vanish. Hence it is concluded that it must have a minimum. The flaw in this reasoning strikes us immediately, since we use the abstract term *function* and are familiar with all the singularities functions can present when the word is understood in the most general sense.

But it would not be the same had we used concrete images, had we, for example, considered this function as an electric potential; it would have been thought legitimate to affirm that electrostatic equilibrium can be attained. Yet perhaps a physical comparison would have awakened some vague distrust. But if care had been taken to translate the reasoning into the language of geometry, intermediate between that of analysis and that of physics, doubtless this distrust would not have been produced, and perhaps one might thus, even to-day, still deceive many readers not forewarned.

Intuition, therefore, does not give us certainty. This is why the evolution had to happen; let us now see how it happened.

It was not slow in being noticed that rigor could not be introduced in the reasoning unless first made to enter into the definitions. For the most part the objects treated of by mathematicians were long ill defined; they were supposed to be known because represented by means of the senses or the imagination; but one had only a crude image of them and not a precise idea on which reasoning could take hold. It was there first that the logicians had to direct their efforts.

So, in the case of incommensurable numbers. The vague idea of continuity, which we owe to intuition, resolved itself into a complicated system of inequalities referring to whole numbers.

By that means the difficulties arising from passing to the limit, or from the consideration of infinitesimals, are finally removed. To-day in analysis only whole numbers are left or systems, finite or infinite, of whole numbers bound together by a net of equality or inequality relations. Mathematics, as they say, is arithmetized.

III.

A first question presents itself. Is this evolution ended? Have we finally attained absolute rigor? At each stage of the evolution our fathers also thought they had reached it. If they deceived themselves, do we not likewise cheat ourselves?

We believe that in our reasonings we no longer appeal to intuition; the philosophers will tell us this is an illusion. Pure logic could never lead us to anything but tautologies; it could create nothing new; not from it alone can any science issue. In one sense these philosophers are right; to make arithmetic, as to make geometry, or to make any science, something else than pure logic is necessary. To designate this something else we have no word other than *intuition*. But how many different ideas are hidden under this same word?

Compare these four axioms: (1) Two quantities equal to a third are equal to one another; (2) if a theorem is true of the number 1 and if we prove that it is true of $n + 1$ if true for n, then will it be true of all whole numbers; (3) if on a straight the point C is between A

and B and the point D between A and C, then the point D will be between A and B; (4) through a given point there is not more than one parallel to a given straight.

All four are attributed to intuition, and yet the first is the enunciation of one of the rules of formal logic; the second is a real synthetic *a priori* judgment, it is the foundation of rigorous mathematical induction; the third is an appeal to the imagination; the fourth is a disguised definition.

Intuition is not necessarily founded on the evidence of the senses; the senses would soon become powerless; for example, we can not represent to ourselves a chiliagon, and yet we reason by intuition on polygons in general, which include the chiliagon as a particular case.

You know what Poncelet understood by the *principle of continuity*. What is true of a real quantity, said Poncelet, should be true of an imaginary quantity; what is true of the hyperbola whose asymptotes are real, should then be true of the ellipse whose asymptotes are imaginary. Poncelet was one of the most intuitive minds of this century; he was passionately, almost ostentatiously, so; he regarded the principle of continuity as one of his boldest conceptions, and yet this principle did not rest on the evidence of the senses. To assimilate the hyperbola to the ellipse was rather to contradict this evidence. It was only a sort of precocious and instinctive generalization which, moreover, I have no desire to defend.

We have then many kinds of intuition; first, the appeal to the senses and the imagination; next, generalization by induction, copied, so to speak, from the procedures of the experimental sciences; finally, we have the intuition of pure number, whence arose the second of the axioms just enunciated, which is able to create the real mathematical reasoning. I have shown above by examples that the first two can not give us certainty; but who will seriously doubt the third, who will doubt arithmetic?

Now in the analysis of to-day, when one cares to take the trouble to be rigorous, there can be nothing but syllogisms or appeals to this intuition of pure number, the only intuition which can not deceive us. It may be said that to-day absolute rigor is attained.

IV.

The philosophers make still another objection: "What you gain in rigor," they say, "you lose in objectivity. You can rise toward your logical ideal only by cutting the bonds which attach you to reality. Your science is infallible, but it can only remain so by imprisoning itself in an ivory tower and renouncing all relation with the external world. From this seclusion it must go out when it would attempt the slightest application."

For example, I seek to show that some property pertains to some object whose concept seems to me at first indefinable, because it is intuitive. At first I fail or must content myself with approximate proofs; finally I decide to give to my object a precise definition, and this enables me to establish this property in an irreproachable manner.

"And then," say the philosophers, "it still remains to show that the object which corresponds to this definition is indeed the same made known to you by intuition; or else that some real and concrete object whose conformity with your intuitive idea you believe you immediately recognize corresponds to your new definition. Only then could you affirm that it has the property in question. You have only displaced the difficulty."

That is not exactly so; the difficulty has not been displaced, it has been divided. The proposition to be established was in reality composed of two different truths, at first not distinguished. The first was a mathematical truth, and it is now rigorously established. The second was an experimental verity. Experience alone can teach us that some real and concrete object corresponds or does not correspond to some abstract definition. This second verity is not mathematically demonstrated, but neither can it be, no more than can the empirical laws of the physical and natural sciences. It would be unreasonable to ask more.

Well, is it not a great advance to have distinguished what long was wrongly confused? Does this mean that nothing is left of this objection of the philosophers? That I do not intend to say; in becoming rigorous, mathematical science takes a character so artificial as to strike every one; it forgets its historical origins; we see how the questions can be answered, we no longer see how and why they are put.

This shows us that logic is not enough; that the science of demonstration is not all science and that intuition must retain its rôle as complement, I was about to say, as counterpoise or as antidote of logic.

I have already had occasion to insist on the place intuition should hold in the teaching of the mathematical sciences. Without it young minds could not make a beginning in the understanding of mathematics; they could not learn to love it and would see in it only a vain logomachy; above all, without intuition they would never become capable of applying mathematics. But now I wish before all to speak of the rôle of intuition in science itself. If it is useful to the student, it is still more so to the creative scientist.

V.

We seek reality, but what is reality? The physiologists tell us that organisms are formed of cells; the chemists add that cells themselves are formed of atoms. Does this mean that these atoms or these

cells constitute reality, or rather the sole reality? The way in which these cells are arranged and from which results the unity of the individual, is not it also a reality much more interesting than that of the isolated elements, and should a naturalist who had never studied the elephant except by means of the microscope think himself sufficiently acquainted with that animal?

Well, there is something analogous to this in mathematics. The logician cuts up, so to speak, each demonstration into a very great number of elementary operations; when we have examined these operations one after the other and ascertained that each is correct, are we to think we have grasped the real meaning of the demonstration? Shall we have understood it even when, by an effort of memory, we have become able to repeat this proof by reproducing all these elementary operations in just the order in which the inventor had arranged them? Evidently not; we shall not yet possess the entire reality; that I know not what which makes the unity of the demonstration will completely elude us.

Pure analysis puts at our disposal a multitude of procedures whose infallibility it guarantees; it opens to us a thousand different ways on which we can embark in all confidence; we are assured of meeting there no obstacles; but of all these ways, which will lead us most promptly to our goal? Who shall tell us which to choose? We need a faculty which makes us see the end from afar, and intuition is this faculty. It is necessary to the explorer for choosing his route; it is not less so to the one following his trail who wants to know why he chose it.

If you are present at a game of chess, it will not suffice, for the understanding of the game, to know the rules for moving the pieces. That will only enable you to recognize that each move has been made conformably to these rules, and this knowledge will truly have very little value. Yet this is what the reader of a book on mathematics would do if he were a logician only. To understand the game is wholly another matter; it is to know why the player moves this piece rather than that other which he could have moved without breaking the rules of the game. It is to perceive the inward reason which makes of this series of successive moves a sort of organized whole. This faculty is still more necessary for the player himself, that is, for the inventor.

Let us drop this comparison and return to mathematics. For example, see what has happened to the idea of continuous function. At the outset this was only a sensible image, for example, that of a continuous mark traced by the chalk on a blackboard. Then it became little by little more refined; ere long it was used to construct a complicated system of inequalities, which reproduced, so to speak, all the lines of the original image; this construction finished, the centering of the arch, so to say, was removed, that crude representation which

had temporarily served as support and which was afterward useless was rejected; there remained only the construction itself, irreproachable in the eyes of the logician. And yet if the primitive image had totally disappeared from our recollection, how could we divine by what caprice all these inequalities were erected in this fashion one upon another?

Perhaps you think I use too many comparisons; yet pardon still another. You have doubtless seen those delicate assemblages of silicious needles which form the skeleton of certain sponges. When the organic matter has disappeared, there remains only a frail and elegant lace-work. True, nothing is there except silica, but what is interesting is the form this silica has taken, and we could not understand it if we did not know the living sponge which has given it precisely this form. Thus it is that the old intuitive notions of our fathers, even when we have abandoned them, still imprint their form upon the logical constructions we have put in their place.

This view of the aggregate is necessary for the inventor; it is equally necessary for whoever wishes really to comprehend the inventor. Can logic give it to us? No; the name mathematicians give it would suffice to prove this. In mathematics logic is called *analysis* and analysis means *division, dissection.* It can have, therefore, no tool other than the scalpel and the microscope.

Thus logic and intuition have each their necessary rôle. Each is indispensable. Logic, which alone can give certainty, is the instrument of demonstration; intuition is the instrument of invention.

VI.

But at the moment of formulating this conclusion I am seized with scruples. At the outset I distinguished two kinds of mathematical minds, the one sort logicians and analysts, the others intuitionalists and geometers. Well, the analysts also have been inventors. The names I have just cited make my insistence on this unnecessary.

Here is a contradiction, at least apparently, which needs explanation. And first, do you think these logicians have always proceeded from the general to the particular, as the rules of formal logic would seem to require of them? Not thus could they have extended the boundaries of science; scientific conquest is to be made only by generalization.

In one of the chapters of 'Science and Hypothesis,' I have had occasion to study the nature of mathematical reasoning, and I have shown how this reasoning, without ceasing to be absolutely rigorous, could lift us from the particular to the general by a procedure I have called *mathematical induction.* It is by this procedure that the analysts have made science progress, and if we examine the detail itself

of their demonstrations, we shall find it there at each instant beside the classic syllogism of Aristotle. We, therefore, see already that the analysts are not simply makers of syllogisms after the fashion of the scholastics.

Besides, do you think they have always marched step by step with no vision of the goal they wished to attain? They must have divined the way leading thither, and for that they needed a guide. This guide is, first, analogy. For example, one of the methods of demonstration dear to analysts is that founded on the employment of dominant functions. We know it has already served to solve a multitude of problems; in what consists then the rôle of the inventor who wishes to apply it to a new problem? At the outset he must recognize the analogy of this question with those which have already been solved by this method; then he must perceive in what way this new question differs from the others, and thence deduce the modifications necessary to apply to the method.

But how does one perceive these analogies and these differences? In the example just cited they are almost always evident, but I could have found others where they would have been much more deeply hidden; often a very uncommon penetration is necessary for their discovery. The analysts, not to let these hidden analogies escape them, that is, in order to be inventors, must, without the aid of the senses and imagination, have a direct sense of what constitutes the unity of a piece of reasoning, of what makes, so to speak, its soul and inmost life.

When one talked with M. Hermite, he never evoked a sensuous image, and yet you soon perceived that the most abstract entities were for him like living beings. He did not see them, but he perceived that they are not an artificial assemblage, and that they have some principle of internal unity.

But, one will say, that still is intuition. Shall we conclude that the distinction made at the outset was only apparent, that there is only one sort of mind and that all the mathematicians are intuitionalists, at least those who are capable of inventing?

No, our distinction corresponds to something real. I have said above that there are many kinds of intuition. I have said how much the intuition of pure number, whence comes rigorous mathematical induction, differs from sensible intuition to which the imagination, properly so called, is the principal contributor.

Is the abyss which separates them less profound than it at first appeared? Could we recognize with a little attention that this pure intuition itself could not do without the aid of the senses? This is the affair of the psychologist and the metaphysician and I shall not discuss the question. But the thing's being doubtful is enough to justify me in recognizing and affirming an essential difference between

the two kinds of intuition; they have not the same object and seem to call into play two different faculties of our soul; one would think of two search-lights directed upon two worlds strangers to one another.

It is the intuition of pure number, that of pure logical forms, which illumines and directs those we have called *analysts*. This it is which enables them not alone to demonstrate, but also to invent. By it they perceive at a glance the general plan of a logical edifice, and that too without the senses appearing to intervene. In rejecting the aid of the imagination, which, as we have seen, is not always infallible, they can advance without fear of deceiving themselves. Happy, therefore, are those who can do without this aid! We must admire them; but how rare they are!

Among the analysts there will then be inventors, but they will be few. The majority of us, if we wished to see afar by pure intuition alone, would soon feel ourselves seized with vertigo. Our weakness has need of a staff more solid, and, despite the exceptions of which we have just spoken, it is none the less true that sensible intuition is in mathematics the most usual instrument of invention.

Apropos of these reflections, a question comes up that I have not the time either to solve or even to enunciate with the developments it would admit of. Is there room for a new distinction, for distinguishing among the analysts those who above all use this pure intuition and those who are first of all preoccupied with formal logic?

M. Hermite, for example, whom I have just cited, can not be classed among the geometers who make use of the sensible intuition; but neither is he a logician, properly so called. He does not conceal his aversion to purely deductive procedures which start from the general and end in the particular.

(*To be continued.*)

DISCONTINUOUS VARIATION IN PEDIGREE-CULTURES[1]

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HEREDITY may be defined as that appurtenant function of living matter by which qualities, characters and capacities are transmitted through successive generations. The absolute identity, or measurable expression, of the inherited characters may be qualified by partial and individual fluctuations about a norm in a continuous series in the progenies, and these characters, singly or in groups, may be subject to dominations, recessions, integrations and resolutions in hybridizations, or to various forms of combination, actual or apparent. In the case of discontinuous variations or mutation, single or unit characters, or constellations of them, may be activated, or converted into a latent or perlatent condition.

Methods of Investigation

Owing to the stimulus of recent discovery, attention is focused at the present time upon the ultimate result of fluctuating variability as influenced by various agents, in the origin and fate of hereditary strains, species, races or physiologically unified groups of organisms, and upon the probable part played in the matter by the saltatory movements, which are being brought to notice so plentifully in all quarters. In connection with the last-named feature, the behavior of the individual qualities or unit-characters in hybridizations are being studied with enormous zeal as offering a ready analysis of the action of inheritance. The comparative ease with which hybrid combinations of plants are effected, and the simplicity of the subsequent resolutions in the progeny, render this phase of the subject easy of attack, and results are being obtained, which, if one may judge from recent literature, do not seem available to many writers.

It needs but a moment's consideration to bring the realization that the entire subject offers some of the most abstruse and difficult problems in the whole realm of biological science. Intricate and elusive in their physiological complexity, we may hope to uncover the main factors by perfected methods in research upon the ultimate mechanical basis of heredity coupled with a refinement of technique in dealing with the course of inheritance as we trace it from generation to

[1] Lecture given at Marine Biological Laboratory, Woods Holl, July 20, 1906.

generation, eliminating guesswork as to parentage, and prophecy as to offspring. The first is steadily yielding results of undoubted importance and is bringing about a renewed interest in the functional relations of the components of the protoplast with respect to the inheritance of characters. The second method, that of statistical observations and experimental methods in pedigreed cultures, has given such notable results in the hands of various investigators, that it may be truly asserted that it may not be outclassed in value by any form of research yet used in investigations in natural history.

As an explorer, do you wish to ascertain the source, direction, character, rate of flow and confluence of a river across your route? Surely, you do not reasonably attempt it by an examination of a single reach, or from a photograph of a single waterfall. Even so surely you may not gauge the possibilities of development, or estimate the potentiality or method of action of groups of characters, embraced in a hereditary strain, guided by dimly recognized forces for thousands of years, by the measurement of a preserved specimen. Physiological problems demand analytic methods of observation of living material.

What ridicule might we not heap upon a botanist who attempted to make a study of geotropism from consideration of the dried material in a herbarium. The existence of such a form of reaction might indeed be recognized, but what futile inferences might be drawn as to its mechanism or the nature of the results. It seems unnecessary and superfluous to call attention to a generalization so obvious; yet that the necessity is not lacking is shown by the material that crowds the pages of our technical magazines and popular periodicals.

Inadequate Treatment of the Subject

Before proceeding with the main thesis it will be profitable to notice some of the most glaring of the inadequacies of treatment which have been recently exhibited, and to call attention to certain unsupported statements which so far have gone unchallenged.

A vice-president of the American Association for the Advancement of Science, in a recent address, has taken occasion to call up the mutation theory, and assumes to have given it a test by "reexamining certain groups of birds and mammals, of which I had previously made systematic studies, for the purpose of discovering evidence, if such exists, of the formation of species by mutation." This author says that "for a quarter of a century I have been an earnest field student of plants in relation to geographic environment. These studies have convinced me that with plants as with animals the usual way in which new forms (subspecies and species) are produced is by gradual progressive development of minute variations." In regard to this comprehensive statement I may say that I have read practically every-

thing that Dr. Merriam has put into print concerning his extensive and thorough field work in the west, and that I yield to none in my admiration of the wide inclusiveness of his results and the profitable manner in which he has treated incidentally the general features of the occurrence and distribution of plants. This appreciation is heightened by the fact that I have spent many seasons in the regions covered by him during the last fifteen years, and that I have carried on experimental work in the field and at the Desert Laboratory with many of the species which are included in his generalizations. I have not been able to come upon the evidence, or record of evidence, upon which his sweeping statements relative to plants are based, although detailed studies upon the relation of plants to environmental factors have been in progress for some time.

Dr. Merriam does not find any evidence to support the conclusion that species arise by mutation. It would be a matter of great surprise if he had. It would be as reasonable to have demanded of him the solutions of problems of respiration from his preparations and field notes. Once a mutant has appeared, no evidence of its distribution can be taken to account conclusively for its origin. Although I have had many mutants under experimental observation, I should not be able to speak with reasonable certainty as to the origin of any of them, had I not ascertained it by guarded pedigree-cultures. It also follows that the systematists who announce and describe new forms as mutants, simply from preserved specimens, or from individuals, the origin of which is not a matter of careful observation, are following a wholly unwarranted practise. Several months ago I had occasion to say "that the 'naturalists,' as some zoologists term themselves, having made the greatest number of essays to offer a universal interpretation of the problems of distribution, are to be credited with the greatest number of defenseless assumptions. In all genetic and evolutionary researches too much emphasis can not be laid upon the basal fact that the physiological and morphological natures of the two great classes of living things are so widely divergent that the derivation of universal biological principles from their apparently concurrent behavior must be made with the greatest caution." Nowhere does this find better exemplification than in the unedifying results of a recent discussion of isolation as a factor in evolutionary action. A number of zoologists have assumed to speak of the distribution of plants, with apparently no basis except 'general information' to the effect that closely related species do not have the same habitat. This has been variously put, but the general meaning is as given. Now such a conclusion is so widely inapplicable, and is so loosely guarded, as to be wholly without value as a statement of a principle in plant

geography and floristics. That the actual mechanical contiguity of two forms competing for the same conditions of habitat would result in some stress is to be taken for granted, but this vegetative struggle would by no means be severe enough in any case to eliminate one from any region. If the advocates of the idea that closely related species do not occupy the same region take this ground on the assumption that hybridization disturbances would follow, here again would be an unwarranted assumption. Readiness of hybridization is by no means a measure of consanguinity, and any slight difference of habit, so small perhaps as not to be capable of description, might ensure pure fertilization. In the case of forms differing by one or a few characters, Mendelian splitting might operate to maintain the forms even if hybridization did occur.

The accompanying photograph of *Opuntia fulgida* and *O. mammillata* presents two forms so closely related that the latter has been taken as a variety of the former by some botanists, but it has been found to be a distinct and physiologically unified strain, and worthy of specific rank. These two forms are widely intermingled, though of course not many instances of actual contact such as appears in the illustration are to be found. The flora in the vicinity of the Desert Laboratory, at Tucson, presents scores of similar examples among other species representing many genera.

Aside from such misinterpretations, a prolific source of confusion lies in the widely different conceptions as to the nature of the taxonomic units used in zoological and botanical writings, as a consequence of which we have some zoologists calling attention to the supposed fact that certain botanists of differing views have no real conception as to the nature of 'species' and 'varieties.' Such statements serve the useful purpose of emphasizing the disadvantageous prejudices under which their authors labor.

While such misunderstandings contribute to hinder progress and confuse the subjects, the basal and underlying fault consists in the fact that taxonomic and geographic methods are not in themselves, or conjointly, adequate for the analysis, or solution of genetic problems. The inventor did not reach the solution of the problem of construction of a typesetting machine by studying the structure of printed pages, but by actual experimentation with mechanisms, using printed pages only as a record of his success. Likewise no amount of consideration of fossils, herbarium specimens, dried skins, skulls or fish in alcohol may give any actual proof as to the mechanism and action of heredity in transmitting qualities and characters from generation to generation, although from such historical data the general trend or direction of succession may be traced.

Misinterpretation of Results

Beyond such mistaken attempts at an analysis of the problems, there is another series of difficulties that interferes materially with the advancement of knowledge of the subject. This consists in careless, prejudiced or mistaken interpretation of results, having the force in some instances of actual misrepresentation. Such demonstrations do no final harm, yet they befog a difficult subject: with 'opinions' and 'beliefs,' they are quite out of place in any scientific discussion.

Since I have had *Œnothera Lamarckiana,* one of the plants which offer a favorable example of discontinuous variation in unit-characters, under cultivation for several years, I am disposed to regard Dr. D. S. Jordan's recent statement concerning this plant as of the above character. He says "it is not at all unlikely that the original *Œnothera Lamarckiana* found in the field near Amsterdam was a hybrid stock, a product of the florist, the behavior of its progeny being not unlike that which appears in the progeny of hybrids. It is moreover known that the seeds of hybrids of an American species, probably *Œnothera biennis,* the common evening primrose, with other American species produced by Mr. Burbank, have been in the past years sold in the cities of Germany." The well-informed botanist will be in doubt as to whether these statements are supposed to lie in 'the plane of ether,' or are to be taken literally. If seriously meant, carelessness as to the existence of records of the plant in question in several localities, long previous to the beginning of the activity of any living horticulturist, is shown. Moreover, material from these localities has been found to be in a mutative condition. It is unnecessary to cite facts so readily accessible in any well-appointed botanical library. The following will be of interest in connection with the statements quoted above:

1. Numerous and repeated hybridizations between *O. biennis* and other species have been made without obtaining anything resembling *O. Lamarckiana* in anatomy or behavior. Several unit-characters are exhibited by *O. Lamarckiana* not found in any other species.

2. *O. grandiflora* obtained pure in its original habitat is now giving off mutants in the cultures in the New York Botanical Garden, after a manner generally similar to *O. Lamarckiana.*

3. The close examination of the evening-primroses shows that the several species are extremely localized. *O. grandiflora,* discovered by John Bartram in 1778, was unknown except in gardens, until rediscovered in the original habitat by Professor S. M. Tracy in 1904. *O. parviflora* has been known in Europe at least since 1759 and has not been seen in America until 1905, when some undoubted indigenous specimens were found in Maine. During the interim the reasoning

shown above would have designated these two species as 'hybrid' or 'garden products.'

It is true, of course, that similar finality of evidence with regard to *O. Lamarckiana* has not yet been obtained, but it does not seem 'unlikely' that it may come to hand when we are able to organize a search for it as well-directed as for the species named. Collectors are few in the region to be covered, and it might be many years before it could be traced to its habitat, even if it occupied a large area. To those interested in suppositions, however, the following from Miller ('Figures of the Most Useful and Uncommon Plants described in the Gardener's Dictionary exhibited on Three Hundred Copper Plates Accurately Engraven after Drawings taken from Nature,' Vol. II., 1760) will suggest the need of caution in the matter. He says regarding the "Tree Primrose with oval spear-shaped indented Leaves, and Flowers proceeding from the Wings of the Leaves on the Upper Part of the Stalk":

> This plant is also a Native of *North America;* but was the first species of the Genus which was brought to Europe, so is more commonly seen in the Gardens than any of the other species. In some Parts of *Europe,* this is spread about from the Gardens in such Plenty, that it might be supposed a Native there. In a small Wood near *Haerlem* in *Holland* this Plant covered the Ground insomuch that many skilful Persons supposed it was a Native of that Place. But it may be easily accounted for; because the Gardeners who live near that Place, are chiefly Florists, and they annually change the Earth of the Beds in their Gardens; so by carrying out of their old Earth from their Beds, in which many of the Seeds were scattered the Plants came up there; and those being suffered to scatter their Seeds, had filled the whole Wood with the Plants.
>
> This differs from the first Sort, (described and figured as *O. biennis*) in having broader Leaves; the Stalks grow taller, and the Flowers are much larger. Both these Sorts will thrive in the Smoak of London better than most Plants.

The appended descriptions and the plate (No. 189, dated 1757) very fittingly characterize *O. biennis* and *O. Lamarckiana,* and as the descriptions were made before *O. grandiflora,* the only known species which might be confused with *O. Lamarckiana,* was discovered, this evidence is serious enough to give one pause in ascribing a hybrid origin to the last-named. Meanwhile the mutation-theory, based on the conception of unit-characters, will neither stand nor fall by suppositions or proof as to the ancestry of *O. Lamarckiana.* If this plant and all of its derivatives were obliterated from our records, the facts in our possession well warrant current conclusions as to unit-characters and their appearance and disappearance in hereditary strains in saltatory fashion.

A certain literary freedom of expression is well illustrated by the following citation from a recent article by Dr. Jordan in this magazine, in which he says:

In Mr. Burbank's cross of the English walnut (*Juglans regia*) with the California walnut (*Juglans Californica*) the first generation shows a certain blending of the traits of one species with those of the other. In the next generation appears every conceivable kind of variation in every feature of the plant and in every function of its organs.

The last sentence offers a fair example of the misrepresentation to which Mr. Burbank's horticultural work has been so profusely the object. A similar progeny of a hybrid oak is included in experimental cultures in New York, and the observer may readily see that the physiological possibilities are not exhausted in either case. To illustrate the possible variations in form would require many millions of individuals, as may be seen when a simple computation shows that seven single differentiations would require more than sixteen thousand individuals for their exemplification, if the characters behaved as indivisible units. If, however, qualities or characters are capable of modification or variation, as indicated by the quotation, the number of different forms of any organ of the entire plant would be so large as to make estimates useless. Then again by what extended experimentation have the conceivable variations in every function been ascertained?

A wider range of literary license prevails in some recent articles by Mr. E. A. Ortmann. Among other inaccuracies he says:

De Vries failed entirely to take notice of this fundamental principle (intergradation), and to show that his elementary species and his mutations are not connected by intermediate forms.

Although somewhat familiar with 'Die Mutationstheorie' and 'Species and Varieties,' no explanation occurs to me to account for this mistaken statement. A few combination forms were found and faithfully recorded by de Vries, but these were certainly not intergrades, whatever might be said of them.

Mr. Ortmann's discussions introduce a novel feature, in his estimate of the futility of experimental methods, which has the sole merit of boldness, coming at a time when the greater number of workers in the subject are turning from discussions and statements of opinion to actual observations. A mistrust is shown by him of experiments 'under artificial and unnatural conditions, as for instance in the botanical garden, or with domesticated forms.' Several months ago the following characterization of this attitude was given in a paper on the subject:[2]

Popular belief in the influence of environment and the inheritance of acquired characters finds its commonest expression in 'that plants have been changed by cultivation.' Domesticated races are spoken of as 'garden forms' by botanists and horticulturists, with the implication that they are specialized types resulting from the effects of tillage. Now so far as actual cultivation is concerned, this assumption is without foundation, since at the present time

[2] 'Heredity and Origin of Species,' *Monist*, January, 1906.

no evidence exists to show that the farm, garden or nursery has ever produced alterations which were strictly and continuously inheritable, or were present, except under environic conditions similar to those by which the alterations were induced, although vague statements and erroneous generalizations to the contrary are current. It it true of course that structural and physiological changes may be induced in a strain of plants in any generation, which may persist in a share to the second, or even in some degree to a third, but no longer.

So far nothing has been offered which would tend to disprove these conclusions.

It is by no means intended to maintain that the stream of heredity may not be altered by the action of external agents, and the possibility of having such changes ensue in experimental cultures beckons with alluring finger to the observer. So important do I hold this aspect of the matter, that a series of experiments, yet in progress, were begun previous to the mutation cultures, and these tests have been continued and expanded until one plant is now undergoing culture in New York, Jamaica, and in connection with the Desert Laboratory, at sea-level and altitudes of 2,300, 5,000, 6,000 and 8,000 feet, and under conditions widely different from those prevalent in its original habitat. If at the end of the decade, this, or any other of the species under test, shows any transmission of the characters induced by the various localities, the care and work necessary in the experiments will be richly rewarded. In this comment reference is had to factors presented by tillage, or entering into the environment of plants in their native habitats.

Announcement has been previously made that mutations, breaks, saltations or discontinuous action may be caused in inheritance by forces external to the protoplasts and cells, which are the true bearers of the hereditary characters (see p. 17).

The technique of the methods by which such changes are induced might be simulated by the action of gaseous emanations, from the soil, radio-action, foreign pollen, or by the stings and incisions of insects, but certainly these possible factors would be as potent with wild as with cultivated plants, as may be seen in the description of the manner in which such changes have been produced.

The Method of Pedigree-cultures

The importance of pedigree-cultures of plants as a means of tracing the course and action of heredity has been notably emphasized by recent investigations, and it will be profitable to go into a detailed statement of the manner in which experiments of this kind are carried out. One of the most striking developments of methods of research in botany has been the continually increasing extent to which taxonomists are having recourse to observations upon growing material in botanical gardens and experimental grounds. In the determination of

difficult problems of relationship, it is becoming more and more customary to secure the individuals representing the doubtful forms and cultivate them under identical conditions, thus securing data for comparison and analysis, representing all stages of development of the sporophyte from the seedling to the mature fruit. The record of important questions which have been solved in this manner is a long one, and includes the investigations of Alexis Jordan on *Draba,* Sargent on *Cratægus,* Wittrock on violets, Britton and Rose on *Crassulaceæ* and scores of other less extensive researches.

If the observer becomes interested in the hereditary action of his plants in addition to a comparison of their anatomical and physiological qualities, it then becomes necessary to follow his plants from generation to generation to ascertain to what extent and in what manner variation may ensue.

The first step in this work is to secure purely fertilized seeds. Hybridization is not common among plants except in a few genera, yet in tests which must continue for a number of years every precaution must be used to ensure accuracy of results. The observer, therefore, covers the unopened flower buds of the individuals from which he wishes to procure seeds with bags of paper, or other suitable tissue, and then makes sure that pollination is secured spontaneously, or by hand, with no danger of admixture of any kind.

In due time the ripened seeds, with photographs, notes and proper herbarium material, are taken from the parental individuals. With the first lot of seeds on hand, the next step is to make a pure culture from them. To do this a quantity of soil of the proper consistency is secured, and while in a moist condition is heated to the boiling point of water in an oven on two succeeding days, or, better still, to a higher temperature in an autoclav for four or five hours. The treated soil may now be stored to be used as wanted, but at all times it must be guarded from possible contamination by the introduction of foreign seeds.

Seed-pans, of earthenware, or shallow wooden boxes are next secured and thoroughly washed in clean water and filled with sterilized soil, after which they are set in place in a cold frame, or in a greenhouse or germination chamber. As each pan is to be used it is taken to a special operating table, and the selected seeds are sown directly from the packet, so that from three to five hundred (in the case of small seeds) are evenly distributed over the surface. A thin even layer of earth is sifted over the seeds, a wooden label is affixed to the pan, giving all necessary data, and the pan is returned to its place in the culture room. If more than one species is being tested at the same time, the greatest care must be used to prevent admixture, and the remote separation of the pans may be necessary. The splashing of

FIG 1. SEED-PANS AND SEEDLINGS IN PEDIGREED CULTURES IN PROPAGATING HOUSE, NEW YORK BOTANICAL GARDEN.

water from a hose, the contact of the nozzle or of the spout of a sprinkler, the careless brushing of the sleeve or the hand against the dirt, may result in the transference of seeds and the vitiation of the results of years of labor and care, especially if a complicated series of tests is under way.

Having observed all the above precautions, the seeds finally sprout in due time, unfold the seed-leaves and begin development. The remote possibility is to be taken into account that the parental individuals may have been hybrids and that this, the second, generation may illustrate the resolutions, combinations, dominancy and recessivity of the ancestral characters. If, however, no such differentiation be encountered the observer has before him a progeny which by the multiplication of his seed-pans may be made to include as many individuals as might be found in a great geographical area during any season. The accurate examination of this material, and of that offered by succeeding generations may reveal evidence of the highest interest, bearing upon various problems in heredity. It is to be noted that if guarded seeds were not obtained for the beginning of the test, the more important work must await the second generation under culture for its beginning.

In even the first examination of the progeny of any physiologically unified strain it will be evident that some diversity of form and appearance is present. This variability is generally of the fluctuating type, that is, the entire number of individuals present may be arranged in a series with respect to any quality. Thus, if the one with the narrowest leaf-blades and the one with the widest are placed at opposite poles, the

others may be arranged in a continuous series which shades by the smallest increments from one to the other. In making such an arrangement, it will be found that the greater number resemble, or lie near, an individual or group of individuals at some place along the line between the two extremes. Statistical measurements of this or any other quality may be made, and the position of the norm of the type determined accurately, as well as the range of variability. In like manner, the correlation between any two characters or qualities may be worked out. Now, having in hand such a mass of data from the initial culture, a fair basis is had for taking up various questions. Thus a succeeding generation might be grown in soil deficient of any nutritive factor, or poor in all of them, in rich soil, or in a substratum highly charged with any element, or at unusual temperatures for the purpose of determining the extent and manner in which the range of variability may be altered by such special treatment, and this may be accentuated by the continuance of the test in successive progenies.

Then again the pedigree-culture offers a fair opportunity for a demonstration of the influence of the effects of selection upon the range of variability and the mean value of any quality. Comparative cultures of seeds taken from the widest-leaved individuals with those of the narrower leaves will tend to show the result of such selection, especially if the selection is continued through several generations. In all these tests it is assumed that the seeds for the following generations should in all instances be guarded as directed above and sown in sterilized soil. With such close series of cultures the question of self- and close-fertilization might arise, and here again the culture affords invaluably exact material for a test of a subject upon which but little definite information exists.

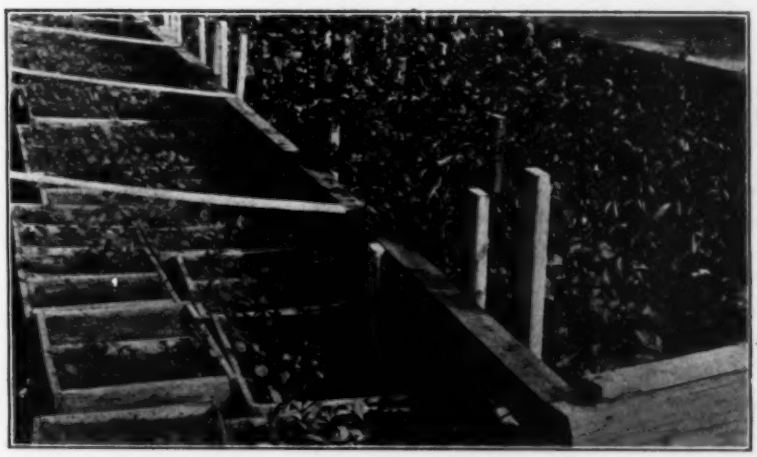

FIG. 2. SEED-PANS AND PLANTLETS OF *Crataegus*, IN PURE CULTURES, ARNOLD ARBORETUM.

The possibilities of the pedigree-culture are by no means exhausted with covering the above points, especially if the plants chosen for the objects happen to be capable of vegetative reproduction, that is by cuttings or slips, in which case comparisons should be made of progenies grown from seeds of the plant with wide leaves with individuals grown from slips of the same, and also compared with others from the extreme end of the series.

It is by undiscriminating discussions of horticultural operations involving seed-selection and hybridization, followed by vegetative propagation, that the public mind has become confused as to the nature and

FIG. 3. EXPERIMENTAL GROUNDS SHOWING PEDIGREED CULTURES OF *Œnothera* IN NEW YORK BOTANICAL GARDEN.

possibilities of selection, notably by the 'popular' and pseudoscientific descriptions of the thorough work of Mr. Burbank. By successive selections certain features, such as size or quality, of a fruit are forced to a maximum development perhaps in a single plant, or may be in hundreds, after which the desirable quality is carried along, or propagated, in quantity by cuttings of the original plant, thus excluding a possibility of a return to the average habit of the species.

Or, in hybrid combinations and resolutions, the desirable constitution of some horticultural form may be secured only after the most highly complicated and repeated crossing, with a result too complex to be easily analyzed. With one desirable individual at hand which produces nuts, berries, cherries, apples, potatoes or plums, or timber, it may be made to produce hundreds and thousands exactly like it merely by using its buds and branches for grafting and budding or propagating.

The use of seeds of the desirable form, either from the original or from any of its derivatives, might give play to all the complex activity of the splitting of hybrids and of the free play of fluctuating variability.

During the course of study of the fluctuating variability of a species, by means of successive generations grown from purely fertilized seeds, the observer may be so fortunate as to encounter individuals which do not fit into his series by reason of the possession of some quality not visible in the greater number of the progeny, or by the lack of such qualities. Thus in a progeny of red-flowered plants one may be encountered which has white bloom, or an individual with laciniate leaves may come in a pure progeny the remainder of which has entire leaves,

FIG 4. *Opuntia fulgida* AND *O. mammillata* GROWING IN CONTIGUITY IN THE VICINITY OF DESERT LABORATORY, TUCSON, ARIZONA.

or a wholly glabrous specimen may be in a hairy progeny, or an individual may depart from the progeny in all these particulars. In either case it is apparent that the variability here is not one of the modification of a character, but by the total accession or loss of a character, and the variability is therefore a discontinuous one. In the progenies in which such variants have been seen hitherto, they form a proportion never larger than six or eight per cent. of the whole number and generally in much less quantity. If such mutants or saltatory variants be found, they should be closely followed, as they may furnish the observer with facts of the greatest value. Care should be taken to secure purely fertilized seeds and a minute anatomical examination should be made of the entire plant, with special respect to the characters in which it appears to differ from the mass of the fluctuating variants around the type.

The seeds secured from such mutants should be sown as directed above, and as many individuals as possible secured for comparison with the parental type, which should be continued as before. If now the individuals of the mutant progeny are placed in a series with respect to any given quality, statistical observations may show whether it is included within the range of fluctuating variability of the parental type. The question therefore as to whether a plant is a continuous or discontinuous variant is one of simple measurement and estimation of qualities, not a matter of opinion. With that simple question easily disposed of, the investigator may next turn his attention to the correlations, a phase of the question of vivid interest, since the sparse data at hand seem to point to a wider range of variation and less degree of correlation in such mutants than in the parental type. That is to say, instead of mutants being derived from forms showing widely fluctuating variability, we have them appearing in the progenies of species in which the range of variation is small and the correlations close, while the derivatives themselves swing through a wide range of fluctuations.

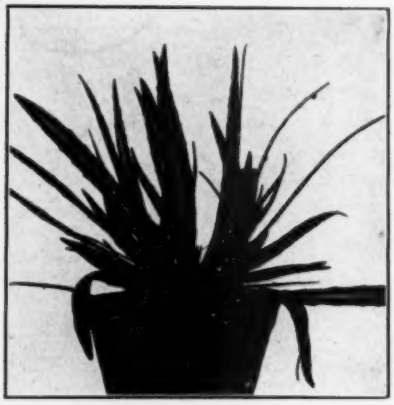

FIG. 5. ROSETTE OF *Raimannia odorata*.

As a complementary means of investigation of the constancy and independence of the various characters dealt with in selection and mutation as observed above, hybridization forms an invaluable means of analysis, and is to be constantly resorted to in all stages of the various phases of the work, since in the observation of the behavior of unit-characters in combination and after resolution the clearest appreciation of their character may be reached.

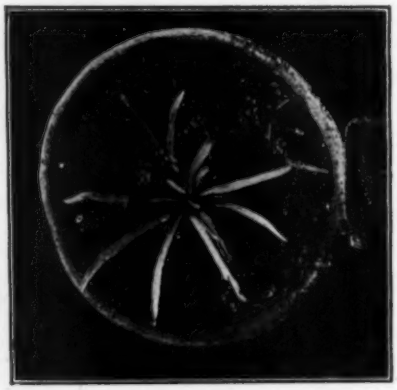

FIG. 6. ROSETTA OF MUTANT OF *Raimannia odorata* INDUCED BY CHEMICAL TREATMENT OF OVULES.

Still another phase of variability is that in which the greater portion of an individual will be found to comply with the requirements of a continuous series, but which bears a branch or portion of a branch which differs notably by the loss or acquisition of characters. Although all

discontinuous variations or mutations are essentially vegetative, yet these are generally termed so exclusively. It is to be noted that such sports, or mutants, when closely fertilized, come true to their aberrant characters. Among other numerous conclusions sustaining this point, one which has recently come to notice again is that of Kerner,[2] who said:

> The fact is that all plants may at some time sooner or later produce an aberrant branch, which differs from the parental type in many characters, and numerous phenomena force us to the conclusion that under the conjunction of favorable circumstances such aberrants would become the starting points of new species.

In the context the author is careful to point out the limitations of such a method of origin of forms.

FIG. 7. *Œnothera biennis*, THE COMMON EVENING PRIMROSE.

Actual pedigrees from such sports or aberrant branches have been tested, in one instance by de Vries and in two others by myself, with the unanimous result that they were found to be constant to their aberrant characters.

Having carried on such pedigree-cultures with a large number of species for several years and having encountered some which did and others which did not give rise to aberrant individuals, attention was directed to the possibility of inducing changes in the hereditary elements in such a manner that the qualities transmitted would be altered or destroyed. A theoretical consideration of the subject seemed to indicate that the changes constituting the essential operation of mutation ensued in a stage previous to the reduction divisions in the embryo-sac, or the pollen mother cells. It was planned therefore to subject

[2] 'Die Abhängigkeit der Pflanzengestalt von Klima und Boden,' Innsbruck, 1869.

these structures to the action of chemical agents, not ordinarily encountered by the elements in question, at a time before fertilization occurred. The tests were planned to include the use of a solution of high osmotic value, and mineral compounds, some of which are toxic in concentrated solutions and stimulating in the proportions used. The probability of success would be heightened with the number of ovules contained in any ovary operated upon, and therefore the common evening-primrose, *Œnothera biennis*, *Raimannia odorata* a relative of it and a member of the same family, *Begonia*, *Cleome*, *Abutilon*, *Sphæralcea* and *Mentzelia* and others were experimented upon. Without recourse to the detail of the work, it may be stated that the use of radium preparations, sugar solutions (10 per cent.), and solutions of calcium nitrate one part in one to two thousand of distilled water with capsules of *Raimannia odorata*, and zinc sulphate in a stronger solution used with *Œnothera biennis*, was followed by very striking results. In the first-named plant, there appeared in the progeny obtained from a few capsules of one individual, several individuals which were seen to differ notably from the type with the appearance of the cotyledons, and, as development proceeded, it was evident that a mutant had appeared following the injections and nowhere else, and thus to have some direct relation to the operation. The characters of the newly arisen form were so strikingly aberrant as to need no skill in their detection. The parent was villous-hairy, the mutant entirely and absolutely glabrous, the leaves of the parent have an excessive linear growth of the marginal portions of the leaf-blades and hence become fluted; the excess of growth in the mutant lies along the midrib and the margins become revolute. The leaves are widely different in width, those of the mutant being much narrower. The parental type is of a marked biennial habit and near the close of the season the internodes formed are extremely short, which has the result of forming a dense rosette; the mutant forms no rosette by reason of the fact that the stem does not cease, or diminish its rate of elongation, and hence presents an elongated leafy stem, which continues to enlarge as if perennial. The first generation

FIG. 8. MUTANT OF THE COMMON EVENING PRIMROSE, INDUCED BY TREATMENT OF OVULES WITH SOLUTIONS OF ZINC SULPHATE.

of the derivative came to bloom at the beginning of the present year, and bare mention of the existence of the derivative was given in a lecture before the Barnard Botanical Club at that time. The real value of the changes induced however lay in the transmissibility of the newly exhibited qualities. The flowers of the mutant were closely guarded and as soon as seeds were obtained these were planted to obtain a second generation. A few plants were obtained, which in every particular conformed to the new type and exhibited no return to the parental type.

Injections of the ovaries of *Œnothera biennis* were followed by the production of one individual, which was recognizably different from the parental type in many qualities, some of which were plainly apparent even in the earliest leaves of the seedlings. These differences have become accentuated with the adult plant and are graphically illustrated by figures 7 and 8. The succeeding generations of this mutant are yet to be tested. The parental form has been under observation for five years in cultures and in a wild condition. An aberrant form, which appears to be ever-sporting, has been previously figured, and while this form appeared in the injected or treated seeds in a normal proportion, yet the newest aberrant has not been seen elsewhere. The probability must be taken into account that it may be a mutant of rare occurrence, the cycle of which came within the experiments, but in either case it is plainly a mutant, and it only remains to be seen whether or not it was induced by the action of the zinc solution. The presumption seems to favor such an affirmative conclusion.

In finding our way about in the voluminous literature of evolution it must be borne in mind that the subject embraces the origin and development of the universe, and that it has engaged the serious attention of workers in all branches of knowledge. The multiplicity of viewpoints has resulted in the greatest diversity of conclusions as a necessary concomitant of widely differing methods of approach to the subject. Much that has been written concerning the subject is of a purely literary or polemical character, embodying prejudices, general opinions and beliefs, putting forward conclusions drawn at long range from attempted interpretations of the results of investigations not properly considered, and brought out for the sole purpose of swaying opinion or influencing sentiment. All work of this character as well as narrow and insecurely founded investigations are futile and ineffectual except to befog the subject and hinder progress.

The problems included in a study of organic evolution are essentially physiological, and the elucidation of the mechanism and action of heredity by which qualities, characters and capacities are transmitted from generation to generation may be accomplished only by accurate observations and experimental tests with active or living material. The examination of preserved material not in hereditary series, or the wide generalizations derived from geographic studies, may not con-

tribute to the progress of exact knowledge of genetics, or methods and manner of inheritance.

The combined and organized efforts of all the botanists in the world concentrated upon all the herbaria in existence would add but little to existing conclusions upon this subject, if we may judge by past achievements or immediate promise, while the most precise information upon geographical distribution can be of interest only in deciphering what has been accomplished, what forms exist and where, the factors influencing their movements, and where these have probably originated.

To appreciate the mechanism of heredity an exact knowledge of the nature and behavior of the bodies which form its physical basis must be gained. To ascertain the action of heredity, statistical and accurate observations must be made upon long series of pedigreed progenies, and these must be carried out in such manner that environmental conditions may be either controlled or their influence measured. Pedigree-culture, first extensively applied by de Vries to clovers, teasels, poppies, snap-dragon and evening-primroses with such marked success, and now used by many workers with animals as well as plants, has proved to be one of the most efficient forms of research yet used by the biologist, and its usefulness is hardly beginning to be realized. The various phases of selection, the accurate measurement of fluctuating variability, correlations, the amount and character of the influence of environmental factors, the effects of close and cross breeding, and the detection of saltatory variations may be accomplished under circumstances which allow a thorough and exact appraisement of the general physiological value of such phenomena by the use of cultures in continuous series.

While the phases of evolution are generally estimated in terms of origin or formation of species, the basal problems of heredity are not especially concerned with the taxonomic estimation given by this author or that author as to the taxonomic standing of any form, nor does it matter whether it is a subspecies, elementary species or 'real' species. The questions of evolution are to be answered by the acquisition of more accurate knowledge concerning the accession, modification or loss of functions, capacities and characters of physiologically unified groups of organisms, regardless of the necessarily more or less artificial appraisements of taxonomy. The briefest review of recent literature will reveal the widest diversity of opinion between botanists and zoologists, and also unexplainable differences among botanists and zoologists as to the species-conception. The value of discussions into which such possible differences may enter is not enhanced by this fact.

The carrying out of pedigree-cultures in New York has revealed the occurrence of discontinuous variants or mutants in *Œnothera biennis*, and *O. grandiflora* among the species tested, in addition to furnishing ample exemplification of the derivatives of *O. Lamarckiana*, as described by de Vries. Mutations in other genera await further

test and observation, a matter which may occupy some time, before final announcement.

Despite general assertions to the contrary, no evidence has yet been obtained to prove that the influence of tillage, 'cultivation,' or the mere pressure of environmental factors has produced any permanent changes in hereditary characters of unified strains of plants.

The above is not meant as a sweeping assertion that inherited characters may not be affected by agents external to the protoplasts that bear them. On the contrary, the experiments now well advanced and conclusively verified, first announced in December, 1905[4], and here described for the first time, show that saltatory inheritance has been induced by the action of external agents upon the ovules of two species of seed-plants.[5] The alterations in question consist in the total suppression of some qualities of the parental form and the substitution therefor of new characters or of a total gain of new qualities in some instances, and the differentiating points between the parental form and the derivative are both anatomical and physiological.

The atypic form which has been tested to the second generation in one species is found to constitute a mutant in the sense in which that term is used by de Vries, and is a real and actual departure from the course of the hereditary strain. The capacity of the mutants induced in this manner for survival would depend entirely upon the environment into which they might be thrown.

If we seek a similar possible intervention of external forces which might act upon the plant unaided by man, we might find such influence coming from radio-active substances, such as spring- and rain-water, or from the effects of sulphurous and other gases which are being set free in numberless localities, or the protoplasts most nearly in contact with the egg-apparatus may well excrete substances which would produce the same effect, without regard to the forces which originally caused the disturbances in the extra-ovular tissue. Lastly it is to be said that the actual technique of injection might be imitated in a measure by the action of foreign pollen which might find lodgment on the stigmatic surfaces, and sending down tubes through the style introduce unusual substances to the vicinity of the egg-cell without participating in normal fertilization, which would ensue in the customary manner. Lastly it is to be said that it would appear that a most prolific source of such disturbances might be expected to result from the stings and lacerations of insects, or the action of parasitic fungi, both sources of the most profound morphogenic alterations in somatic tissues, profusely exemplified by the well-known gall formations of plants.

[4] MacDougal, 'Heredity and Origin of Species.' Reprinted in advance from the *Monist* for January, 1906, and distributed December 18, 1905.

[5] The possibility is not excluded that the reagents may have affected the elongating pollen-tubes.

AMERICA AND SEISMOLOGICAL RESEARCH

By Dr. WILLIAM HERBERT HOBBS

BUT a few years ago, the American naval officer serving his tour of duty upon the European station found in the antiquated vessels in which he was compelled to appear a constant source of mortification. This condition has now passed, and it is the geologist who in his turn is humiliated as the modern European earthquake station is opened for his inspection. A great earthquake upon American home territory has been registered by all first-class seismographs throughout the world, and the records have been collected for comparison and study at central stations. It is the kindest thing to say of the American records that they are a negligible quantity—for measured by modern standards they are—but unfortunately their inclusion in the autograph albums of the California earthquake of 1906 does not allow them to be overlooked. Thus the backwardness of our country in a most important branch of the great science, in which we had perhaps thought ourselves entitled to some respect, is patent to all.

It will hardly be claimed for us that the United States offers no opportunity for earthquake investigation. In 1811 a devastating quake affected a large area in the central Mississippi Valley, in 1872 occurred the great Owens Valley earthquake in Nevada, and in 1887 the Sonora earthquake of even greater violence; not to mention the Charleston and the recent California seistus. Lighter shocks have been frequent, and the greatest of earthquake authorities, the Count de Montessus de Ballore, showed some years ago that New England, the St. Lawrence Valley, the central Atlantic coast generally, the central Mississippi Valley, and above all the Pacific coast of the United States, must be regarded as notable earthquake provinces.

The better to understand our true position, let us consider what has been accomplished in earthquake investigation within the last ten years. First, and most important, the laws of earthquake distribution have been determined, and the relation of earthquakes to topography and geology has given us a new branch of science—seismic geography. This is almost exclusively the work of one man, the Count de Montessus de Ballore, major of artillery in the French army, who has given the better part of his life to this arduous labor.

From a wholly different direction the problems of earthquakes have been approached through the perfecting of seismometrographs, until they register all great seisms of our planet, however distant. This

point reached, an entirely new field has been opened before our eyes. The new autographs of earthquakes have characters dependent upon the distances the waves have traveled to produce the record; so that the observer at a station can unaided give the distance of the disturbance within 50 miles, an error negligible in view of the extended area disturbed.

By combining records made at several widely-separated stations, not only the distance from a given station, but a sufficiently exact location for each quake, is easily obtained. To have developed a great system of some forty such stations, scattered throughout the length and breadth of the globe, is the great service rendered to science by the British Association for the Advancement of Science, especially, and by the leader of its Seismological Committee, Professor John Milne. Thus it has been revealed that the great earthquakes of the planet are twentyfold more numerous than those reported by observation *in situ*, and that most of them occur upon the floor of the ocean, where other methods of observation would have failed to reveal their presence.

The analysis of the complex of waves registered in the seismogram is extending our knowledge of the nature of the earth's interior, and affording the solution of problems which, in importance and in difficulty of approach from other directions, can only be compared with those now being solved by the study of radioactivity.

No attempt to sum up the achievements of seismological research during the past ten years should fail to note the fact that the Japanese have for systematic and thorough study of the general problems, but even more for the practical applications of these investigations to the amelioration of the conditions in an earthquake-tormented country, taken the first place among the nations. Italy, also, with almost a score of stations of the first rank and with two hundred correspondents scattered throughout its small territory (to telegraph the first news of a quaking to the main office at Rome), has played no mean part in the advance of the science.

The center of earthquake investigation upon the continent is now, however, the Imperial German Chief Station for Earthquake Investigation at Strasburg. Professor Gerland, its director, now issues the annual catalogue of earthquakes, and he has the credit of having organized, in 1903, the International Seismological Association, and of having founded its journal, the *Beiträge zur Geophysik*. The work of the station now devolves largely upon his highly-trained assistants, Professor Rudolph and Dr. Sieberg.

The writer assumes that a beneficent result may be expected to follow from the frightful calamity at San Francisco in the stimulation of seismological research in America; so that we may later take our proper share of both the labor and its rewards. The start can

easily be made too hastily. There is much contention over the merits of the different types of seismometrograph, which differ as widely as possible. It is perhaps not strange, in view of the new vistas opened for discovery, that the analysis of the records from some instruments not provided with compensating devices has brought out waves supposed to originate in the earth, which exist only in the vibration periods of the instruments themselves.

America's broad extent and her outlying territories and protectorates (Alaska, Cuba, Porto Rico, Panama, the Philippines, Hawaii, Guam and Tutuila), offer her special advantages for a correlated system of earthquake stations; but she will do well to wait until her principal station has been well established, her type of seismograph determined, and a corps of trained expert observers found. This will require some time, and can be greatly hastened if pride be put aside and some one of the thoroughly trained men available in Europe be invited to superintend the erection of the first earthquake station.

Some sacrifice the pioneer must always make, and so it happens that the English stations are fitted out with a type of instruments already obsolescent. On the point of establishing her outlying stations (German East Africa, Shan Tung, Samoa), Germany will be more fortunate. The maker of scientific instruments for almost the entire world, she has steadily perfected her types before launching upon the larger undertaking. America will have at least the consolation of profiting by the experience of the other nations during the past ten years, and there is need for much study of it.

The recent investigation of earthquakes has thus developed along two somewhat different lines: (1) the macroscopic study upon the ground of felt quakes, undertaken by men trained as geologists; and (2) the microscopic investigation of the distant, large, locally 'unfelt' quakes, undertaken at special earthquake stations by men trained as physicists. There is much need that these different lines of endeavor should be brought into as close a union as possible, for only through mutual support can the best results be achieved. As Dr. Sieberg, the secretary of the Strasburg station, told the writer, the more difficult of the seismograms afford equivocal data if not checked by the reports 'from the field.'

The American Association for the Advancement of Science brings the geologist into association with his brother the physicist, as well as with many other scientists who take an interest in investigations of such general interest as those upon earthquakes. The writer takes this opportunity to urge that the association follow the glorious example of its British cousin and select from its membership a committee to watch over the interests of seismological research in America and to direct the course of legislation in accordance with its teachings.

IMMUNITY IN TUBERCULOSIS[1]

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I CAN not begin this address without delaying a moment to testify to my sense of the great honor which has been conferred upon me by your invitation. Neither can I proceed with it until I have expressed to you my conviction that there are persons present in this audience whose scientific work on tuberculosis makes them far abler than I to discuss the complex problem of immunity in tuberculosis. My work in bacteriology in the past has not led me to an especial consideration of the highly important problem of the prevention and cure of tuberculosis, and I can therefore account in no other way for my selection to address you this evening than that you desired this topic presented to you from the point of view of one who has done some work in the general field of bacteriology.

The modern study of tuberculosis, as you know, begins with the generation which immediately preceded the epoch-making discoveries of Koch. It may, I think, be said with justice that this study was inaugurated by the first purposeful transmission by inoculation of the disease from animal to animal. For whatever may have been the speculations upon the infectious and transmissible character of the disease before this demonstration, yet the demonstration was necessary before further steps in the elucidation of the cause and prevention of the disease could be taken. Koch in his masterful monograph gives the credit of successful inoculation to Klencke, who in the year 1843 succeeded in inducing an extensive tuberculosis of the lungs and liver in rabbits by inoculation with portions of miliary and infiltrating tubercles from man. Klencke, after accomplishing this result, did not continue his investigations and they were consequently soon forgotten. In the meantime Villemin's experimental investigations were begun and pursued to a successful termination. He inoculated not only with tubercular material from human beings, but also from cases of bovine tuberculosis, and he seemed to have proved experimentally the identity of the latter disease with human tuberculosis. Villemin's researches, from the number of his experiments, the careful manner in which they were carried out and the employment of suitable control experiments,

[1] Address delivered at the joint meeting of the Association of American Physicians and the National Association for the Study and Prevention of Tuberculosis, held at Washington, D. C., May 16, 1906.

appeared to decide the question in favor of the infective theory of tuberculosis. The numerous workers who repeated Villemin's experiments, after the same or modified methods, arrived at very contradictory results. The opponents of the infective theory strove to prove that true tuberculosis could be induced by inoculation with *non-tubercular* material. To the decision of this question Cohnheim and Salomonsen contributed largely by selecting for inoculation the anterior chamber of a rabbit's eye. The great advantage which this method possesses over all others arises from the fact that the course of a successful tubercular inoculation can be watched throughout by the experimenter until the pathological process has advanced so far that the whole organism—the neighboring lymphatic glands, the lungs, spleen, liver and kidneys—becomes tuberculous. A further point in favor of this method of inoculation is that spontaneous tuberculosis of the eye has never been observed in rabbits. It was reserved for the genius of Robert Koch to discover nearly twenty years later, in 1882, by the employment first of an original staining method, the tubercle bacillus in sections of tuberculous organs, and next by the use of a special method of artificial cultivation, to secure growths of the bacillus free from all admixture with extraneous matter. With these pure cultivations he succeeded, as you well know, in reproducing in certain domestic animals all the characteristic appearances of tuberculosis in man. Furthermore, Koch's studies of this period convinced him of the unity of causation of the various tubercular affections met with in man and also of those met with in the common domestic animals. Refusing to be daunted by the fact that tuberculosis tends to appear under different aspects in each species, and directing his attention not upon the gross appearances of the disease, but focusing it upon the microscopical appearances of the primary tubercle, which as he said recurs with typical regularity in all the different processes in man, Koch recognized the essential identity of the apparently widely different forms of tuberculosis in the various species of animals. It does not detract from the immense value of his work that Koch failed to distinguish between the tubercle bacilli isolated from the tubercular tissue in fowls, cattle and man. This failure was by no means accidental, for the possibility of the existence of differences in nature of the cultures depending upon their origins was clearly in his mind. Many of you will recall the long list of cultures which is given in the paper on tuberculosis published in 1884. In regard to this list Koch says: "It may cause some surprise that so relatively large a number of cultures was set on foot when a few would have sufficed for observing the behavior of bacilli in cultures. It seemed to me, however, not improbable that though bacilli from varying forms of tuberculosis—perlsucht, lupus, phthisis, etc., presented no differences microscopically, yet, that in cultures differences might become

apparent between bacilli from different sources. But although I devoted the greatest attention to this point, I could find nothing of the kind. In all the cultures, whether taken from miliary tubercles, lupus or perlsucht, the tubercle bacilli behaved exactly the same."

Our knowledge of the nature of the tubercle bacillus has been increased until at this time several distinct kinds are recognized. These may conveniently be classified according to their chief sources into human, bovine and avian tubercle bacilli, and into so-called tubercle bacilli of cold-blooded animals. This last group of bacilli, which will detain us only a short time, differs greatly from the other varieties, as can readily be seen when the fact is recalled that the high temperatures —temperatures approaching blood heat—which are required for the growth of the mammalian and avian bacilli, quite preclude their multiplication under conditions of ordinary external nature. Hence they are not adapted to a life outside the living body except as cultivated artificially at this relatively high temperature. In man's conflict with tuberculosis this fact is of the greatest service, since by reason of it he is enabled to disregard the danger of any increase in tubercle bacilli outside the animal body. The relatively low temperatures at which the tubercle bacilli of cold-blooded animals develop adapt them, indeed, to an independent existence; but, as they are wholly devoid of power to cause disease in warm-blooded animals and as they would appear to have a restricted dissemination even among cold-blooded species, they are of comparatively small importance.

Of far greater consequence is the question whether the disparity which exists between the several kinds of tubercle bacilli derived from warm-blooded animals is a wide one. This question, which at first sight may appear to be chiefly of academic interest, has, in reality, far-reaching practical significance. The close relationship which man bears to domestic animals makes every fact of animal disease of high value to him. And in the case of no animal disease are facts of greater moment than in tuberculosis. Not only is the human race, by reason of its dependence upon the animal kingdom for food, work, etc., exposed to the diseases of animals which are transmissible to man, but domestic animals are also exposed to diseases of human beings. This correlative susceptibility may, therefore, cooperate to produce a vicious circle of events by which infection or the dangers of infection are kept alive and threatening. Hence it is that an effective solution of the problem of limitation of tuberculosis, whether by suppression outright or by suppression through the induction of immunity, must take into account the degree to which tuberculous animals of different species, through direct or more remote association, are a source of danger to one another.

There is no longer any doubt that the avian tubercle bacillus departs considerably from the human and from the bovine types of bacilli.

The early observations of the Italian investigators, Rivolta and Mafucci, have been confirmed and so extended as to give us a fairly comprehensive knowledge of the capacities for pathogenic action, upon different animal species, of the avian bacilli. At the same time painstaking studies of the degree to which birds are subject to inoculation with pure cultures of tubercle bacilli of human origin support the view of diversity in type of bacilli and susceptibility of species. And yet, while fowl react only with slight local lesions, as a rule, to inoculations of tubercle bacilli of human origin, certain mammals have proved themselves fairly subject to experimental inoculation with avian bacilli. While the guinea-pig, otherwise so sensitive to inoculation tuberculosis with the mammalian bacilli, is relatively resistant to the avian variety, the rabbit, which exhibits a marked degree of refractoriness to the human bacilli, succumbs quite readily to the avian bacilli. It is, however, worth noting that the reactions in the rabbit which avian tubercle bacilli call forth do not conform to those observed in tuberculosis in general; there is absence of typical tubercles and caseation, and the chief pathological alterations observed are found in connection with the enlarged spleen.

The literature on tuberculosis contains a small number of references to the cultivation from human subjects of the avian tubercle bacillus. From our present knowledge it may be postulated that avian tubercle bacilli occur rarely in man. Rabinowitsch has, indeed, recently emphasized the occasional occurrence of the avian bacilli in cattle, swine, horses and monkeys; but they constitute a small source of danger in the spread of tuberculous disease among mammals. The parrot, because of its use as a pet and of its susceptibility to the avian bacillus, on the one hand, and of the human bacillus, on the other, is a greater menace to public welfare.

The subject of bovine tuberculosis and of bovine tubercle bacilli is among the most important of all the questions relating to the suppression of tuberculosis. The admirable studies of Theobald Smith established the distinction in type subsisting between certain bacilli of human and of bovine origin. We have come now to regard these types as separate and not to be transmuted, at least not readily under artificial conditions of cultivation, into each other. Into the disputed questions of variation due to environment I can not afford to enter. But I would have you believe that transformations of avian, bovine and human bacilli into each other have probably not been accomplished by experimentation. The cultivation of one variety of bacilli in the body of an alien species has been said to alter profoundly the properties of the bacilli; but the observations upon this point are in my opinion far from convincing. The mere fact that avian and bovine varieties of bacilli preserve their peculiar properties when occurring naturally in

the diseased body of an alien species—man, for example—tends to discredit the experimental transmutations referred to.

Bovine tubercle bacilli are characterized, as ascertained by Smith, by a greater degree of pathogenic power for mammals in general than human bacilli, with which fact is correlated certain peculiarities of cultural and physiological properties serving further to separate the bovine from the human bacilli. The bacilli of mammalian origin are, perhaps, closely related and less removed from each other by the sum of their properties than they are from the avian bacillus. With the few exceptions mentioned all forms of mammalian tuberculosis are caused by either the human or the bovine bacillus.

In view of the general fact that the bovine bacilli show a greater degree of pathogenic action for the lower mammals than the human bacilli, it was natural to assume that bovine bacilli would be powerfully pathogenic for man also. To test this probability directly by experiment is, of course, not permissible. But the belief that tuberculosis in cattle is a menace to man is expressed in the many regulations by which it is aimed to control and prevent the use as food of products derived from tuberculous animals. It was not until Koch's address was delivered in 1901 that any serious doubt existed in the minds of sanitarians and pathologists that tuberculous cattle offered a source of danger to man. The specific knowledge which has accumulated since that date has served to establish the transmissibility in some degree of bovine tuberculosis to the human subject. The inherent difficulty and tediousness of the investigation of the specific types of tubercle bacilli existing in human cases of tuberculosis necessarily limit the total number of instances in which it has been established, beyond peradventure, that the bovine type of bacillus does occur in tuberculous processes in man. In this country the responsibility of refuting the too general statement of Koch has fallen chiefly upon Ravenel and Theobald Smith, whose admirable studies in this direction are of a convincing nature.

If we pause for a moment to consider upon what data Koch based his statement of the independence and non-communicability of tuberculosis in cattle and man, we shall appreciate that, in so far as he dealt with established fact and not hypothesis, he had long been anticipated. That cattle are highly resistant to infection with tuberculous material and tubercle cultures obtained from human subjects can be concluded from the early experiments of Baumgarten, Sidney Martin, Frothingham and Dinwiddie. The most conclusive evidence upon this subject is contained in Theobald Smith's paper of 1898, in which he summarizes his experiments by stating that "putting all the facts obtained by experiments on cattle together, it would seem as though the sputum bacillus can not gain lodgment in cattle through the ordinary channels."

In view of these facts, it is not surprising to find that Koch and Schütz later failed to produce marked or general tubercular infection of cattle by feeding or inoculating directly into the circulation tuberculous materials and cultures of tubercle bacilli of human origin. That this result does not dispose of the entire question at issue, but leaves open the important consideration of the implantation of the more virulent bovine bacilli upon man was, of course, present in Koch's mind, and was met by him by emphasizing the infrequency with which primary intestinal tuberculosis, which is the form of tuberculosis presumably arising from ingested virulent tubercle bacilli, is encountered in human beings. The reports which have appeared since have tended to show that primary tuberculosis of the abdominal viscera, especially in children, is not so infrequent as Koch believed it, and the researches inspired by Koch's address have brought out the important fact, now based upon actual observation under the microscope, that tubercle bacilli may pass through the intact intestinal wall and reach, by means of the lymph current, the mesenteric glands; and have made it seem probable, also, that by entering or being carried into the blood vessels in the intestine the bacilli may be carried to the lungs. When all the known facts of food infection in tuberculosis are assembled, they make quite an imposing array, for they indicate, quite in opposition to the exclusive view expressed by Koch, that tubercle bacilli entering the body with food may be implanted upon the mucous membrane of the mouth, from which, probably, chiefly in the region of the tonsils, they may be carried to the lymphatic glands of the neck and adjacent parts where they develop and produce tubercular disease; or they become implanted upon the intestinal mucosa and pass the epithelial barrier without first causing disease there, and set up lesions in the mesenteric lymph nodes or even be transported by the blood or lymph to the distant lungs; or they may first multiply in the intestine, cause tubercular disease there, and then migrate further, involving the abdominal and thoracic organs.

If I have seemed to tarry too long over this aspect of my subject, I will ask you to consider for a moment in how far the endeavor to limit the spread of tuberculosis among the human race must be influenced by the avenues of infection to which the race is exposed. If we side with Koch in the view expressed in 1901, and reiterated just the other day in his Nobel-prize address, that, as he says, human tuberculosis and tuberculosis in cattle are so distinct from each other that the latter is not to be feared as transmissible to man, at least, as his last utterance puts it, not in a form which comes in consideration in regard to tuberculosis as a *'Volkskrankheit,'* or race disease, then it is only necessary to direct efforts to the suppression of tubercle bacilli of human origin. For, if the danger of infection of surroundings and

healthy individuals is limited to the expectoration of persons suffering from tuberculosis of the lungs and upper air passages, the problem before us, while still very large, is less by a considerable amount than if there must also be taken into account the widely prevalent disease among cattle, swine and other domestic animals. While I do not pretend to speak in terms of great authority, yet it would seem to me that the time is not yet ripe to disregard, in attempting to suppress tuberculosis, the disease in domestic animals. Greatly as I sympathize with the active propaganda which is being made by instruction and material help to protect tuberculous human beings from injuring themselves and others, and greatly as I hope to see promoted the means of caring for the tuberculous in sanatoria, etc., yet I hope that there may occur, at this time, no relaxation in the efforts being made to control the spread of tuberculosis among cattle and to prevent the consumption of infected milk and flesh by man and other animals. That, on the other hand, the suppression completely of tuberculosis among cattle would not be followed by a great reduction in the morbidity due to tuberculosis in man is shown by Kitasato's statistics from Japan. In that country the human disease prevailed with its usual activity at a time when the cattle disease, owing to the absence of cattle, was unknown, and milk formed no appreciable element in the food of children.

In dealing with the complex problem of tuberculosis—a problem whose difficulties enlarge with the continued growth in size of cities—we are materially assisted by the knowledge of the manner in which the virus of tubercle is separated from the diseased body, the conditions of its contamination of our environment, and the avenues through which it endeavors to enter the healthy body. Though it is, perhaps, scarcely to be hoped that a time will arrive when tuberculosis will have become, through precautions against infection, as rare as are to-day smallpox and typhus fever, yet it is a most hopeful result of the crusade against tuberculosis that a marked reduction in the mortality, and probably in the incidence of the disease, has been going on in some countries—as, for instance, in England—for forty years. In New York, the system organized by Biggs has brought about a reduction since 1886 of 35 per cent. in the mortality of the disease; and while in Prussia the mortality was stationary in the decade from 1876 to 1886, since that time a reduction of more than 30 per cent. has been noted. These figures show what may be accomplished in reducing the dangers of infection with tuberculosis by a régime of education, improved conditions of living for the poorer classes, and the segregation in hospitals and sanitoria of any considerable number of the infective tuberculous during the most dangerous period of the disease.

The discovery of the microbic agent of tuberculosis naturally awakened the hope that a specific means of treating and, possibly, of

preventing tuberculosis might now be found. The early years following the cultivation of the tubercle bacillus saw no realization of this hope, and to-day we are still far from the desired goal. However, the prodigious labor which has been expended in the search for a means of protection against infection with the tubercle poison has not been wholly devoid of results.

In an address of this kind it is not practicable to deal with the separate contributions, in detail, which the many workers have made to the subject of immunity in tuberculosis. The most that can be accomplished is to bring together the more important results of all the workers and, after having assembled them, to judge of their value and to consider, possibly, in what important respects they are still imperfect. I can not do better, at the beginning, than to remind you that the successful point of departure has been the discovery that variations in type and in virulence exist among tubercle bacilli. The earlier view which taught that the tubercle bacillus is a micro-organism of uniform and fixed virulence has been shown to be erroneous, first by the discovery of variations according to certain origins, and second by a gradual decline in pathogenic power suffered by certain strains through long cultivation outside the animal body.

The animals which have been of special use for tests of immunity are rabbits, cattle and goats. The guinea-pig, which furnishes an almost ideal animal for the detection of tuberculosis, because of the sensitiveness of its reaction to inoculations with tubercular material, fails, for the same reason, to be a highly suitable animal in which to carry out tests of immunity; and yet it has been employed with some success.

The first important contribution to the subject of experimental immunity in tuberculosis was made by Koch in connection with his researches on tuberculin—a product of the growth in broth of tubercle bacilli, freed from the bacilli and concentrated. In spite of the failure of tuberculin to bring about a favorable issue in all cases of human tuberculosis in which it is administered, it still remains a useful, perhaps the most useful, strictly medicinal agent employed for the treatment of tuberculosis. But the sum of its useful properties is not embraced in its employment as a therapeutic substance: it is also a diagnostic agent of high value, and its action upon the tuberculous organism is so specific and remarkable that it has proved itself of the greatest importance and aid in the effort to unravel the complicated series of biological phenomena which constitute the tubercular state.

It is possible to increase somewhat the resistance of animals to tubercular infection by previous treatment of tuberculin; but this increase is not remarkable. It is possible to bring about arrest of the tubercular process in the infected organism by means of tuberculin;

and in some instances this arrest leads, through the changes induced in the tuberculous tissue by means of the tuberculin injections, directly to cure, or indirectly, through an increased power of resistance and attack on the part of the forces of the organism, to eventual cure. But a high and lasting degree of immunity has never been secured by the use of tuberculin. This fact, disappointing as it was at first, is now easily explicable. Tuberculin does not represent the entire series of forces contained in the bacilli which the body has to resist in preserving itself from infection with tubercular poison. The peculiar principles contained in tuberculin are, indeed, not highly toxic for the normal individual; and our experience in securing immunity to micro-parasites and their products has taught us that where no reaction or response to the introduction of the foreign poison is called forth, no degree of protection to larger doses or more virulent poisons of the same nature is to be expected. Toxic as is tuberculin to the tuberculous organism, it is almost innocuous to the tubercle-free body. It has been found, in keeping with this distinction, that the normal animal shows after tuberculin treatment evidence of the minimal production of the neutralizing or antibody for the tuberculin, which, were tuberculin a direct poison for the tissues, would probably be produced in larger amounts. On account of this absence of action on the normal organism it has been thought that the active principle in tuberculin does not exist in a free state, but occurs in some combination, from which the tuberculous, but not the non-tuberculous, organism can free it, and that the separation takes place in the tubercular foci upon which the specific action of the poison is directly exerted. If this view is correct then the failure of tuberculin to exercise any profound action on the healthy organism is easily grasped.

Increased knowledge of bacterial infection and immunity has taught us that in case of bacteria which invade the depth of the body and produce their peculiar effects by reason of their immediate presence, we can not expect to achieve marked immunity through the use of the soluble gross-products of the parasites. The reaction of the body to the invasion depends not upon the presence in the invader of one set of toxic principles, but of many, some of which are contained in the solid substance of the micro-parasite and do not go over into the fluids in which they multiply. Thus it has been found, in case of certain bacteria, that a degree of immunity or protection which it is impossible to obtain even after very prolonged treatment with the fluid portions of cultures, can be secured quickly when small quantities of the living or even dead microorganism are injected into the body. A high degree of bacterial immunity has been secured up to now for a small number of micro-organisms by vaccination—by the method introduced by Pasteur—for several animal diseases, notably anthrax or splenic fever,

fowl-cholera and black-leg. In these cases the living attenuated micro-organisms are employed.

Neither lasting nor marked immunity in tuberculosis can be obtained by the inoculation of cultures of tubercle bacilli killed by heat, sunlight or other agency. Dead tubercle bacilli are poisonous and bring out a striking reaction of the organism, but this reaction does not confer immunity to subsequent inoculations of the living germ. It may well be that the dead bacilli, especially if reduced to impalpable powder so as to facilitate absorption, may after injection raise the powers of resistance in the organic forces, although the height of the sustained forces is not sufficient to enable the body to throw off completely the living infecting organism. It is easy to prove that the animal organism is modified by the development within it of the tubercle bacilli; and merely disposing of dead bacilli increases its power of reaction against a second injection of dead tubercle bacilli; the second action being much more vigorous than the first. (Theobald Smith.) The experiments of Koch which immediately preceded the discovery of tuberculin clearly demonstrated that tuberculous guinea-pigs into which tubercle bacilli are reintroduced subcutaneously react in a very especial manner. An active inflammatory process develops about the site of second inoculation which eventually brings about the expulsion of bacilli with the exudations; a voluminous slough forms, which, when shed, carries with it a large number of bacilli; and this shedding is followed neither by the formation of a permanent ulcer nor hypertrophy of the neighboring glands, a regular result of the primary inoculation. The tubercular organism reacts in the same manner to dead as to living bacilli; the tuberculous animal has acquired immunity against reinfection or reintoxication by the tuberculous virus, which, however, in no way prevents the first inoculation from becoming generalized and setting up a tuberculosis of almost all the organs.

If we attempt an interpretation of these phenomena we can conclude that the organism, once it is poisoned with tubercle virus, becomes supersensitive to the tubercle poison. This supersensitiveness is displayed in the manner of reaction upon re-inoculation of the tuberculous organism to tuberculin and to dead and living tubercle bacilli. But the organism poisoned with dead tubercle bacilli is not in reality tuberculous; it is, however, sensitized. In keeping with this distinction, it can be said that while the tuberculous organism has acquired a degree of immunity to reinfection, the organism merely poisoned with tubercle bacilli has failed to develop this state of resistance.

The experimental results, which I shall relate to you, upon which are based our belief in the artificial production of immunity to tuberculosis, were all obtained by the use of living bacilli. It would, therefore, seem as if in the course of their residence and development within the

body the immunizing organisms behave differently from those in artificial cultivations. This difference in behavior could be accounted for on the supposition that under conditions of parasitic life, surrounded as the bacilli are with complex fluids and more complex cells, they form, in their growth, products which either are distinct from those which are formed by them in cultures, or these products, in *statu nascendi,* are acted upon and modified by the active and labile ferments in the fluid and protoplasm of cells, with which the growth-products must come into immediate contact. Professor Welch, to whom this variation in behavior of bacteria under parasitic and saprophytic states of existence was fully apparent, endeavored a few years ago in his Huxley lecture to explain the difference in activity of bacteria growing within and outside the body by supposing that in the body they are induced to secrete substances the stimulus to the production of which is absent in the culture tube. However this may be, it is evident that the only form of immunity in tuberculosis which deserves the name has been obtained by the employment for inoculation of living cultures of the tubercle bacillus.

Although the earliest experiments which had for their object the production of immunity in small animals by means of previous inoculation of products of the growth and of attenuated cultures of the tubercle bacillus were published in 1890 (Martin and Grancher, Courmont and Dor), yet, I think, the first really promising, because successful, achievements of this end were made by Trudeau in 1902 and 1903 and by de Schweinitz in 1904.

Trudeau protected rabbits from virulent tubercle bacilli by first injecting them with a culture of bird tubercle bacilli, the subsequent injection of virulent mammalian bacilli being made into the anterior chamber of the eye. The rabbits to be protected were twice injected subcutaneously at intervals of 21 days with cultures of the avian bacilli. About one in four of the rabbits died within three months, profoundly emaciated, but without tubercular lesions. The remaining animals recovered and were apparently in good health, when, with an equal number of controls, they were inoculated in the eye with a culture of mammalian tubercle bacilli. The results are instructive: In the controls little or no irritation following the operation is observed and the eye remains quiescent or nearly so for about two weeks, when the changes described in the early parts of this address manifest themselves. After a few weeks general inflammation of the structures of the eye develops, the inoculation wound becomes cheesy and the eye is more or less completely destroyed. The disease, however, remains usually localized in the eye for many months, and may remain there permanently, depending upon the virulence and number of bacilli injected.

In the vaccinated animals, on the contrary, the introduction of the mammalian bacilli at once gives rise to a marked degree of irritation. From the second to the fifth day the vessels of the conjunctiva become engorged, and evidences of marked inflammation appear in the anterior chamber and on the iris (reaction of immunity). However, at the end of the second to the third week, when the eyes of the controls begin to show progressive and steadily increasing evidence of inflammatory reaction, the irritation in those of the vaccinated animals begins slowly to subside and the eyes to mend. In from six to twelve weeks, in the successful cases, all irritation has disappeared and the eyes present only the evidences of traumatism and inflammation. This experiment leaves no doubt of the protective influence exerted by the first inoculations of the avian bacilli and clearly establishes that related cultures of tubercle bacilli of moderate virulence for an animal species, can afford protection to subsequent inoculation with special and more pathogenic strains of the bacillus. Notwithstanding the fact that, as Dr. Trudeau records, some of the protected animals slowly relapse and the disease resumes its progress, although by almost imperceptible stages, the experiment still shows that protection, not absolute immunity, from tuberculosis may be obtained in rabbits by a species of vaccination.

De Schweinitz in 1894 reported certain experiments which he made on guinea-pigs and cattle. He inoculated the former with a culture of tubercle bacilli of human origin cultivated for about twenty generations in broth. This culture was of a low grade of virulence for these animals, but it served to protect them to such an extent that when they were afterwards inoculated with tuberculous material from a cow they remained healthy, while control pigs injected with the same material became tuberculous and succumbed in about seven weeks. De Schweinitz injected large quantities of human tubercle bacilli into cattle—beneath the skin, into the peritoneal cavity and into the circulation—without injury.

I may, at this time, digress for a moment and leave the more strictly chronological method of presentation, to allude to the set of experiments on the protection of guinea-pigs from tuberculosis, which Trudeau reported to the National Tuberculosis Association at its last meeting. The special merit of this experiment is that it shows the existence of a connection between virulence and infectivity in the germ and its capacity to confer immunity. Unless the bacillus has the power to gain some foothold in the body it affords no protection; if on account of high pathogenic power or virulence, it easily gains a foothold, then it brings about infection. To choose a culture of tubercle bacilli of just the right grade of virulence is one of the conditions, apparently, of successful experiment, as it must also be, in view of this fact, one

of the difficulties of the method. The same difficulty has been encountered in the practical carrying out of this method of immunization in cattle. Several series of guinea-pigs were inoculated with tubercle bacilli as follows: (*a*) with dead bacilli, (*b*) with living bacilli from cold-blooded animals, (*c*) with a culture of human bacilli cultivated artificially for more than twenty years which produces on inoculation no appreciable local lesions and never tends to generalize, and (*d*) another human culture cultivated artificially for more than fourteen years, which still causes in all the pigs slightly enlarged inguinal glands near the site of inoculation, and occasionally brings about slight caseation of the nodes with a tendency to partial generalization of the virus. The dead bacilli and the bacilli from cold-blooded species gave no protection; the second human culture, by reason of its greater invasive properties, protects better than the first, which is almost devoid of power to grow in the animal body. In no case, however, was the growth of the virulent bacilli wholly suppressed.

In man the question of acquired immunity has been answered by many authorities, as far as the main considerations go, in the negative. A large number of well-observed facts demonstrates that a person who has suffered from localized tuberculosis of the lymph glands—scrofula so-called—or other form of local tuberculosis, can not count upon an immunity from pulmonary tuberculosis. And yet it can, I think, be shown by reference to statistics that in man there exists a refractory condition which becomes increased after infection, since the number of persons who have been the victims, at some period of their life, of a tuberculous infection, is very large in comparison with the number who die of this disease, or the even larger number who develop severe forms of it. Hirsch gives the mortality of tuberculosis as compared with deaths from all other causes as 3:22, in other words, tuberculosis claims as victims of death 1 in every 7 persons. This proportion does not, however, express the morbidity from tuberculosis, which is, in reality, far greater than these figures indicate. It is difficult to secure by vital statistics reliable data of the incidence of tuberculosis; but trustworthy observations made at autopsies upon human beings indicate that as many as 90 per cent. of persons, dying from all causes, have at some period of their life been the victims of a tubercular infection. In far the greater number of instances the disease remains fixed in the bronchial or other lymphatic glands or the apex of the lungs and exerts no injurious effect upon the organism as a whole. We may, therefore, fairly conclude that the human organism possesses a strong inherent tendency to overcome infection with the tubercle bacillus. So much can be safely predicated. But whether the suppression of a local infection, such as I have described, gives an increased capacity for overcoming subsequently invading tubercle bacilli remains for the present an open

question. It is certainly not disproved by the facts cited; and some authorities hold fast by the belief that a degree of immunity to tuberculosis may be acquired by man.

In the year 1901, on December 12, on the occasion of his acceptance of one of the Nobel prizes, Behring announced that he was engaged upon the study of artificial immunization of cattle to tuberculosis. In this address the claim was made that a method had been perfected whereby it was possible to vaccinate cattle successfully against tuberculosis. These experiments consisted in the endeavor to immunize cattle by means of tuberculin, other toxins, so-called, from the tubercle bacillus, dead tubercle bacilli, bacilli weakened with chemicals and living, active cultures of the tubercle bacillus. In the four years which have elapsed since this announcement was made a series of monographic papers bearing on this subject has appeared from Behring's laboratory in Marburg. The plan of immunization has, in this time, undergone a number of modifications until now it consists in the inoculation intravenously of young cattle—calves twelve weeks old preferably—with a standard human culture, which is now furnished commercially. A second inoculation of an increased quantity of this culture is injected three months later. Cattle treated in this way are regarded as highly immune and are denominated by Behring as 'Jennerized.' If to them a dose of a virulent bovine culture of tubercle bacilli is given, no permanently bad results follow, although an equal dose of the virulent culture will cause, in an unvaccinated animal, the development of generalized tuberculosis leading, in a few weeks, to death.

In his endeavor to find a culture of the tubercle bacillus which would fulfill the requirement of producing a transient illness and leave protection behind, Behring discovered that not all tubercle bacilli of human origin were without danger to cattle inoculated with them. We were, indeed, not unprepared for this announcement, since, in the first place, we had learned that in some instances tubercle bacilli of the bovine type have been cultivated from examples of human tuberculosis, and, on the other, that not all the bacilli, of any type, exhibit equal degrees of virulence. The culture employed by Behring, although it has now been employed to inoculate several thousand cattle, is said never to have produced severe disturbances of health; even when animals already tuberculous are inoculated the results are not serious: fever lasting several days sets in, the animals may cough, and they may eat less and lose somewhat in weight, but even they return to what is for them the normal.

It would appear that McFadyean is entitled to the credit of the discovery equally with Behring of the immunization of cattle from tuberculosis; and, indeed, there is reason to believe that his results

even anticipated those of Behring. By using for injection first tuberculin and then in succession tuberculin and tuberculous material containing bovine and possibly human tubercle bacilli, McFadyean succeeded in increasing the resistance of several cattle to artificial tubercular infection.

Pearson and Gilliland, 1902, in this country early published accounts of some experiments which they carried out upon the immunization of cattle from tuberculosis. They employed a culture of human tubercle bacilli for producing immunity and found that subsequently the protected animals, as compared with the controls, which all succumbed to the virulent inoculation, either developed no lesions or very inconsiderable ones upon being given large quantities of highly pathogenic bovine cultures. As far as I know these experimenters are the only investigators who have endeavored to carry the principles of the method a step farther, so as to bring about arrest of the disease in cattle already tuberculous. While it is unlikely that such a therapeutic use of 'Vaccination' will ever be made in veterinary practise, the facts are of considerable theoretical interest, especially in view of the somewhat similar means employed to arrest tuberculosis in man.

The immense importance to scientific agriculture of the matter of immunization of cattle from tuberculosis and the even greater collateral interest which the subject has for man, as enlarging the possibilities of immunity even for him, have led to a discussion on the priority of the discovery between Neufeld, a pupil of Koch, and Behring. It would appear from Neufeld's writings that, while working under Koch's direction, he ascertained as early as 1900–1901 that large animals—donkeys chiefly, but cattle also—could be protected from artificial infection with virulent tubercle bacilli, always fatal to control animals, by previous treatment with tubercle vaccine, of which several different preparations were studied. It is not within the scope of this address to apportion the credit of priority; but in any case, assuming the facts to be as stated by the contestants, McFadyean should receive as great credit as either of the others, if not the chief credit. The principle which all the investigators employed is not new in experimental medicine, but has come to us from the genius of Pasteur. It may, however, be said that our knowledge of the tubercle bacillus and its varying activities had by the year 1900 become so much enlarged that the possibility of putting the facts of the newly discovered properties to a practical test of immunity occurred to the several independent workers in bacteriology. There can, I think, be no doubt that Behring deserves the credit of making the protection of cattle from tuberculosis a feasible, practical object of study. This alone is a merit of no small order.

From the mere fact that cattle have been successfully protected from infection by the tubercle bacillus, even under the severest condi-

tions of laboratory experiment, it can not be concluded that they will be equally refractory when exposed to the natural sources and modes of infection. In the laboratory the virulent infectious agent is brought into the animal by injection, under the skin, into the serous cavities or into the circulation, which are avenues through which in the natural disease infection rarely if ever takes place. And while this mode of introduction of the virulent bacilli into the body may, theoretically, be more severe than their introduction into the lungs with inhaled air, or into the stomach through infected stalls and food, yet the profound differences in the defenses of the body with which the bacilli come into conflict, under these different circumstances, may, after all, determine the issue in a manner quite contrary to our expectations. It is, therefore, of the highest interest to learn that in their later tests Behring and his co-workers exposed vaccinated cattle to stalls and herds which were known to be badly infected, with the result that at the time of the report, they had apparently escaped infection. I am enabled through the courtesy of a private communication from Dr. Pearson to state that cattle vaccinated by himself and Gilliland which were kept for two years under natural conditions of infection have not contracted tuberculosis, while the control animals, exposed to the same conditions, have all developed the disease, some dying spontaneously by reason of the severity of the infection. Dr. Pearson also informs me that their experiments indicate that the degree of resistance bears a rather definite relation to the number of vaccinations given the cattle. No cattle vaccinated three times with their standard vaccine—a living culture of tubercle bacilli of human origin—have developed tuberculous lesions even after two years' severe exposure. In their experience, two injections of Behring's vaccine do not always suffice for such heavy exposure as they employed.

As regards the question of duration of the protection, it may be said that Behring, basing his views on results of vaccination made three years before, expressed the belief in 1904 that it would endure during the life of the animal. As young healthy cattle are vaccinated before they fall victims to infected stalls and herds, it would seem as if infected herds might therefore gradually be replaced by healthy ones. The gain, this being true, would be almost incalculable to agriculture.

I am in the fortunate position of being able to bring before you a critical summary of the subjects just presented by one wholly conversant with its practical as well as its theoretical aspects. Through the courtesy of Dr. Leonard Pearson I have been enabled to read the advance sheets of a review on immunization in tuberculosis which will soon be issued from the Phipp's Institute. Dr. Pearson concludes that there appears to be no doubt that different cultures of human bacilli have different immunizing values. Some can not be used at all because

they are of too high and others, possibly, because they are of too low, virulence for cattle. There is also need for comparison in immunizing value of fresh cultures and cultures that have been dried in vacuum and reduced to powder. Some observations appear clearly and strongly to indicate that the fresh cultures are preferable. Although it has been shown that vaccination can be practised so as to be entirely harmless to the animals, yet, on the other hand, it is not always unattended with danger. What is the shortest and most economical procedure for the protection of cattle on a large scale is still to be established. Only prolonged observation of carefully recorded results of vaccinations practised on a large scale can settle this point. The question of duration of immunity is still an open one. It has been shown that the immunity endures a year. To say, at the present stage of the studies, that it will last during the entire life of an animal is to make a statement for which there is no experimental proof. Modes of vaccination, as illustrated by the intervals between the successive injections, differ greatly. Behring recommends an interval of three months, while others have obtained a high degree of immunity by repeated injection at short intervals. As artificial immunity is relative and not absolute it need not excite surprise that the immunity to the tubercle bacilli can be overcome by the injection of large quantities of active bacilli. What is desired in practise is a degree of immunity that will suffice to protect animals from acquiring the disease under natural, and consequently highly variable, conditions. In some herds, where the natural disease prevails in a mild form, a lower degree of immunity may suffice than in other herds in which the disease is more severe and wide-spread. We are, therefore, at the beginning of this complex and highly important subject. These are Dr. Pearson's conclusions.

There is another aspect of this subject which demands attention. When it is recalled that immunity in cattle is obtained by the injection of living human tubercle bacilli the question arises whether this procedure is wholly free from danger to the consumers later of the flesh and milk of these cattle. It would appear that the human bacilli do not excite in cattle the tubercular lesions, in which doubtless the bacilli are so enclosed as to be, to a considerable degree, protected from perishing. It is equally true that as the living micro-organism can not be replaced by dead ones in bringing about immunity, the immunizing process is in some way bound up with their survival and even, possibly, with a restricted multiplication. Hence it is necessary that we ascertain, first, how long the human bacilli survive in the organs of the vaccinated animals, and second, whether they are ever eliminated with the milk of cows. The observations already made upon these points are so few as at present not to be useful for any scientific deductions. But before the method is too implicitly relied upon these questions should be answered.

It is an interesting subject of speculation as to what the result will be when cattle in general, and possibly, man later, shall have been immunized to tuberculosis. Will the race of tubercle bacilli disappear in large measure from the world? This would indeed be a beneficent result. But Dr. Smith has pointed out in a recently delivered address that doubtless host and parasite eventually come to hold a kind of equilibrium to each other, and hence an increased degree of resistance in the former might tend to bring about that selection among the parasites through which races of greatly augmented power for invasion would be produced. If this were true, and he even suggests that the natural process of weeding out the weaker among the human race tends to this result, the parasite would try to keep up with the host as his resistance increased until a point was reached beyond which further enhancement of power was impossible. Would the higher animal or the lower vegetable organism finally claim the victory? We need perhaps at this moment not to relax our efforts to achieve a practical immunity for man as well as for animals because of this future danger. I am not aware that the smallpox germ has increased measurably in virulence since vaccination became general, but I would also add that a century is a small period of time in the life history of any living organism.

Before closing this address I should like to refer briefly to the new interest which has been excited in the use of tuberculin in the treatment of human tuberculosis by reason of the application to the study of tuberculosis of a method introduced by E. A. Wright, of London, whereby it is held that the exact effect of the tuberculin injection can be measured and controlled. The method consists in the determination of the capacity of the blood leucocytes to take up tubercle bacilli when the blood and the bacilli are brought together outside the body in a test tube. Wright and his pupils have worked out the normal power of the blood to cause the englobing of the bacilli; and they have noted a diminution of this capacity in the blood of many persons suffering from tuberculosis. They speak of this englobing capacity of the blood as 'opsonic index,' from the word meaning to prepare—to cater for; since the bacilli must first be prepared by substances in the blood serum before they can be ingested by leucocytes. The injection of tuberculin, when cautiously done, tends to bring about a rise in the tuberculous, of the 'opsonic index,' which Wright believes is a measure of the good done, as an increase in immunizing substances in the blood is the cause of the rise. He also discovered that time is required for the occurrence of the rise and that the immediate result of the injection is a fall of the index—so-called negative phase. This latter must be permitted to pass away and be succeeded by the positive phase before another injection is given. Gradually the 'opsonic index' is driven up in the cases that are favorable to the treatment.

I do not intend to discuss the value to the clinician of this interesting method and Wright's observations based upon it. The subject appears to me to be one of great intricacy and therefore to be approached in a spirit of proper criticism despite its evident allurements. My purpose in mentioning it at all is to bring again to your attention a method of exciting the tuberculous body to put forth an effort at self immunization which is sometimes efficient to a marked degree. It is not the injected tuberculin that accomplishes directly the changes in the condition of the patient, for there already exists, doubtless, an excess of similar poisons in the tuberculous foci in the body. The healthy body, indeed, does not react in this manner and is not to be protected, enduringly, from tuberculous infection by a previous treatment with tuberculin. As Koch's phenomenon shows the tuberculous organism to have developed defenses against subsequent tuberculous infection which the normal body does not possess in equal degree, the employment of tuberculin indicates that the diseased body can be aroused artificially to put forth a stronger effort than its unaided natural forces enable it to make, in order that the disease may be overcome. Herein resides a great principle, an immense power for good, and, consequently, a great hope for future progress in the rational and specific treatment of tuberculosis in man. Efficient efforts at suppression of the causes of tuberculosis, deeper knowledge of the principles of bacterial immunity, are the two forces which in time may stay the ravages of the 'White Death.'

THE PROTECTION OF THE ALLUVIAL BASIN OF THE MISSISSIPPI[1]

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THE hydrographic or drainage basin of the Mississippi River (Fig. 1) is equivalent in area to one third of the United States. Thirty-two states and territories contribute water to the volume of the river; eight of these divisions send water to no other system. The discharge of rivers is not in any proportionate way related to the size of their drainage basins. The potent factors which determine the volume of discharge are the precipitation of rain over the basin and the character of the soil. The upper Ganges has a basin less than one seventh that of the Mississippi. It equals the latter river, however, in the volume of its discharge. The Hoang Ho, with a basin area fifty per cent. only of that of the Mississippi, discharges more than twice as much water into the sea. If the discharge of the Mississippi proportionally to the size of its basin equaled that of the Po, the volume of the discharge of the former would be multiplied by six. The Danube more nearly equals the Mississippi in the ratio of discharge to size of

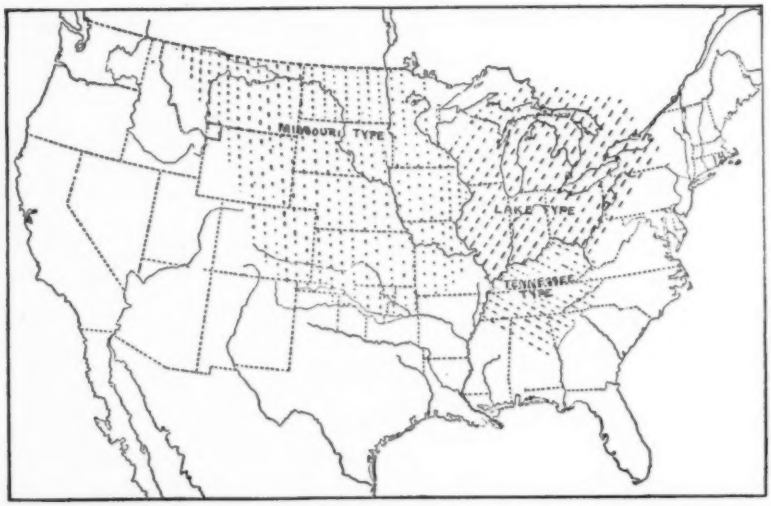

FIG. 1. HYDROGRAPHIC BASIN OF THE MISSISSIPPI, WITH RAINFALL TYPES. Missouri type has the maximum rainfall in April, May and June; Tennessee, in February and March; Lake, a late spring maximum in June, and an early fall maximum in September.

[1] Compiled, largely, from the reports of the Mississippi River Commission.

basin. It, too, has a portion of its drainage area protected from the prevailing winds and approximates to the rainfall condition of the Mississippi. The Ganges and the Hoang Ho are in regions of copious seasonal rainfalls.

The portion of the Mississippi Basin subject to inundation includes land on both sides of the river south of Cape Girardeau, Mo., and aggregates 29,700 square miles, or an area equivalent to the state of South Carolina. Through this alluvial basin (Fig. 2) the river winds in a rather tortuous path, the distance by river (1,700 miles) being nearly three times as long as a straight line drawn from Cape Girardeau to the Gulf of Mexico (600 miles).

It is characteristic of many flood plains that the land immediately adjacent to the river is higher than the more distant parts of the plain (Fig. 3). These higher parts are called natural levees by some writers. If, therefore, the river rises beyond the limit of its banks, there is likely to be a general inundation of the alluvial basin. This feature of flood-plain form is further illustrated by the tributaries of the lower river. From an ordinary valley slope a river flows fairly direct into the trunk stream. If a tributary crosses a flood plain of some size, the stream must force an entrance against the rise of the back slope of this plain and finally must breach the banks of the river.

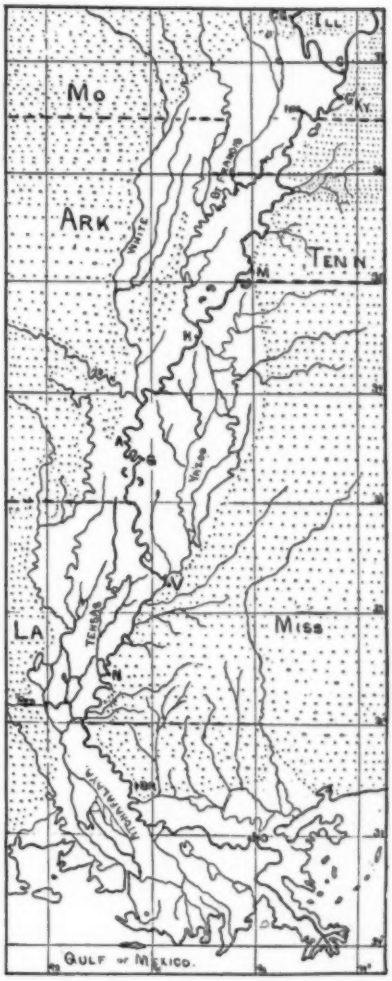

FIG. 2 THE LOWER MISSISSIPPI, WITH TRIBUTARIES, ALLUVIAL BASIN, AND HIGHER LANDS (dotted). *CG*, Cape Girardeau; *C*, Cairo; *C'*, Columbus; *M*, Memphis; *H*, Helena; *A*, Arkansas City; *G*, Greenville; *V*, Vicksburg; *N*, Natchez; *BR*, Baton Rouge; *NO*, New Orleans.

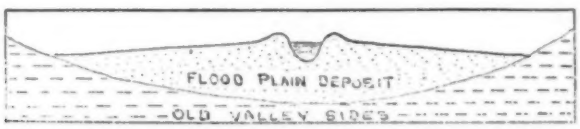

FIG. 3. CROSS SECTION OF A RIVER VALLEY.

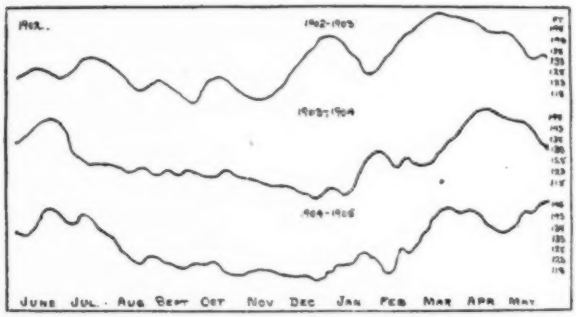

FIG. 4. HYDROGRAPH OF THE MISSISSIPPI RIVER FROM JUNE, 1902, TO MAY, 1905.

It is apparently easier for the tributaries of the lower Mississippi not to overcome this rise, but to flow down stream in the back swamp lands or bottoms until some more formidable barrier forces them to empty their waters into the main channel. In this manner the St. Francis (Fig. 2) flows for 100 miles and is forced into the Mississippi just above Helena, where the main river, after crossing the alluvial basin, touches the higher land on the west. The Yazoo River is forced in at Vicksburg, after flowing along the flood plain for 200 miles. Other tributaries show this same characteristic. The Atchafalaya pursues its course to the Gulf as an independent stream. The height of the river bank over the back swamp districts varies from 10 to 25 feet.

The large area of the drainage basin of the Mississippi would yield an unmanageable amount of water to the lower river during the stage of flood if the excess of discharge at that time resulted from a uniform rainfall condition. The very size of the basin with the tributaries of the main stream reaching far into rainfall areas of different types and seasons is beneficial to the control of floods. Reference to the map (Fig. 1) and the appended explanation may aid one in understanding the condition of rainfall over the basin. In addition, it is well to bear in mind that the condition of the ground affecting the amount of run-off of water is an important factor in the amount of discharge. The Ohio basin has its heaviest rains in January, February and March. Its largest tributaries, the Cumberland and the Tennessee, rise in regions of copious winter rainfall and add enormous volumes of water to the Ohio. The basin of the Ohio is less than one half that of the Missouri, yet it furnishes over twice as much water to the Mississippi. The melting of the snow and the frozen condition of the ground which increases the percentage of run-off at the time of the early spring rains swell the volume of the Ohio, while the late spring rains over the Missouri basin fall on an absorptive soil. Only 15 per cent. of the rainfall is drained from the latter basin and 24 per cent. from the former. The percentage over the Ohio during the flood months because of the conditions stated above is probably much higher.

The sequence of floods from the tributaries is first, the Ohio, then the upper Mississippi, followed by the Missouri and the western streams. Great floods are not annual occurrences. Recently the years marked by excessive floods have been the years of 1893, 1897 and 1903. The floods of the years 1898 and 1904 did not fall much short of the records of the seasons previous.

The hydrographs of floods (Fig. 4) and the profiles of rivers at different seasons (Fig. 5) show that the floods proceed down stream somewhat as a wave. The highest point or crest marks the extreme danger limit due to height of flood. The hydrograph also indicates that the crest flattens somewhat in its down-river progress. The reason for this may be seen in the river profiles which are drawn from three stages of the river's annual fluctuations. The down-stream slope (*AC*) of the flood wave has increased over the normal slope of the river (*GE*). This results in an increase of speed of the waters on this slope. On the other hand, the up-stream slope (*BA*) is a less one than the normal slope (*DG*), and a decrease of speed over normal flow results. At certain times the slope (*BA*) may be against gravity, and a further retardation will be experienced. This would occur when a tributary added a large volume of water to the Mississippi, as in the case of the Ohio during the spring freshets. The tendency of this increase of speed on the down-river side of a flood wave and the decrease of velocity on the up-river side is to reduce the size of the flood wave by draining off the excess of water faster than it can accumulate. If the river is long enough to allow this proc-

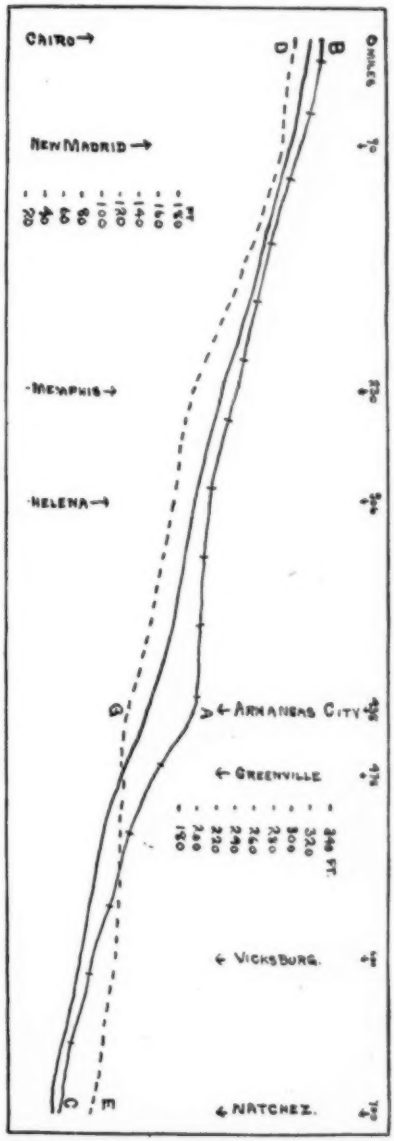

FIG. 5. PROFILES OF THE MISSISSIPPI RIVER AT FLOOD STAGE (*BC*), at low water stage (*DE*), and at an intermediate stage.

ess to so act, a flood would finally reduce itself to an insignificant rise of water.

The following table may yield additional information concerning flood conditions:

Station.	Miles below Cairo.	High Water 1903.	Low Water 1895.	Difference.	Standard High Water.	Highest 1903.	Above Danger Line 1903.	
							At or Above.	5 Feet Above.
Cairo	0	328 ft.	278 ft.	50 ft.	50.17 ft.	50.60 ft.	24 days	8 days
New Madrid	70	302	263	39	42.90	39.50	53	8
Memphis	230	210	169	41	41.60	40.60	54	13
Helena	306	199	145	54	54.10	51.00	67	22
Arkansas City	438	156	99	57	56.30	53.00	82	42
Greenville	478	144	92	52	50.50	49.10	42	12
Vicksburg	599	104	46	58	55.00	51.80	45	21
Natchez	700	74	22	52	54.00	50.40		
Red River Landing	765	60	10	50	52.50	50.00		
Baton Rouge	833	47	7	40	43.20	40.00		
Carrollton	957	27	7	20	20.35	19.40		

Levees were constructed by the early settlers of Louisiana about the year 1717 near New Orleans. In 1844 the right bank of the river in this state was embanked and many other isolated levees were in existence, especially along the Yazoo basin front. The average height of the Louisiana levees was four feet. Through the years up to 1882, there seems to have been a constant agitation of the question of levee building. Riparian states, districts and owners formed committees and boards, taxed the protected lands or the products of these lands and in general managed to put their constituents under a heavy burden of debt. In 1858 a tax of 10 cents per acre was demanded on all lands which were freed from inundation, and such lands as bordered the river were often subjected to a tax of 25 cents per acre. In 1865 a board was constituted with a revenue derived from a tax on cotton of 1 cent per pound. In the year 1882, a flood greater than any previously experienced overflowed the entire basin and destroyed most of the levees then existing. The Mississippi River Commission, created a few years before, now entered upon its work at an opportune time. With the landowners disheartened, their labors resulting in little gain, their money invested in levees swept away, the allotments of the commission were enough to revive the courage of the riparian proprietors. There has been a steady gain in the protection of the alluvial basin since the creation of this commission. At present the levee system comprises about 1,500 miles of structure and is 71 per cent. completed.

The height of the levees is a varying one. If the levee is built on the immediate banks of the stream (Fig. 3), which is the highest part

[2] High water, 1903, and low water, 1895, are reckoned from the Memphis datum. The numbers are, roughly, seven feet too high for Gulf levels as the base.

of the flood plain, a levee less high might suffice as well as one built on the back-slope. Where the original four-foot levee stood in Louisiana is a levee about 16 feet high. The top is not much higher than the top of the original levee, but, being situated at some distance from the banks, its base is 10 feet lower. The immediate banks of the river are so subject to caving that they do not make, in all cases, a safe foundation for a levee. It has been considered safer and wiser in many instances to build a more stable foundation. Furthermore, if the levees are built upon the higher and immediate parts of the flood plain, the levees would be nearer each other. The nearer they approach one another, the higher they must be. Any detraction from the horizontal expansion of the waters must be evidenced in a vertical expansion. With the levees placed further apart, a less height is possible. The thing that determines the height to which they shall be built is the flood. There is an endeavor to place the grade of the levees at from 2 to 4 feet above the gauge measurements of the highest floods. Thus for a while the provisional grade was 2 to 3 feet above the 1897 flood. Later the 1903 flood set a new mark and a grade 2 to 2.5 feet above the 1903 high-water line is suggested. At Lake Providence, the 1903 water mark was 2 feet above the 1897; at Greenville, it was 2.4 feet above. Accordingly, the high water of 1903 reached approximately to the tops of the levees suggested after the 1897 flood, and a new height, 2 to 3 feet above the former, is now demanded. This latter height is about 5 feet above the provisional grade of five years ago. As the levees approach completion, higher and higher grades must inevitably result. As long as there is relief of the waters by incompleted levees and crevasses, the necessary height can not be determined. When the remaining 29 per cent. of levee is constructed and the system withstands one of the greater floods without a crevasse, the excessive flood will determine the height of the levees. Even then certain factors of flood conditions may so unite as to cause a flood which will overtop the system. The statement of the character and condition of the system under the strain of the 1903 flood indicates that there is much left to be done. In the upper part of the basin, the levees were reported too low and of insufficient dimensions. The high water reached to the top for one half of the entire length of the lower St. Francis district and for many miles was above the tops, being restrained from spreading over the basin by capping the levees with planks, dirt and sand-bags. In the district below, it is reported that because of the settlement of the embankment and because about 20 miles of the line in the lower end of the district had not been raised to the provisional grade, considerable work was required to prevent the water from overflowing the levees. Again in Louisiana, the topping of the levees by planks and sand-filled bags was necessary over a distance of 71 miles of the line in order to prevent a wash-over. An engineer reports that

the most vulnerable feature is the instability of the foundation along much of the levee line.

The efficiency of the levee system is the test of all the labor and the justification of the large expenditures. The increasing efficiency may be measured in many ways. A comparison of the number of crevasses and the total number of miles of destroyed levees with the records of previous floods is the one in general use. In 1882, 284 crevasses were recorded and 59 miles of levee were destroyed. This record has been gradually improved. In 1890, but 23 crevasses were reported and 4.25 miles of levee destroyed. In 1897, the number of crevasses increased to 49, with a loss of 8.3 miles of levee. In the 1903 flood, 9 crevasses of importance were recorded and 5 of these caused a loss of 2.1 miles of the levees. The loss of levees by caving banks was a little less than 1 per cent. of the entire contents. Last year (1904) in a period of quieter flood the percentage was over 2.5. The number of square miles of overflowed area in 1903 was .5 the mileage for 1897. There is no doubt but that the levee system as it approaches completion is being made stronger and safer. Yet each crevasse or natural break spreading the confined waters over larger areas releases the tension on the banks and to some extent prevents others from occurring. To enclose the water which has spread naturally over 29,700 square miles between two walls less than 5 miles apart and covering about one tenth of its former area is no easy task. Till the present system is completed, the possibility of a flood will be uncertain that always grave dangers may be incurred to life and property within the limits of the alluvial basin of the river.

The commissioners in their general report furnish the best proof of the increased confidence in the levees by citing the progress and growth of the Yazoo Basin since their board was created. The population of this district was 94,672 in 1880, and 195,346 in 1900. The present valuation of the basin is $42,000,000. The number of banks have increased from 2 in 1893 to 51; the mileage of railroads from 225 in 1884 to 816. The cotton production in 1879 was 185,868 bales; in 1903, 426,414. The increase in corn, peas, clover and alfalfa is reported to be even greater than that of cotton. The original timber of the basin is being cleared, and there are now large shipments of lumber, as logs, boards, staves, headings and the like. Flourishing crops are seen to-day where in former years the floods measured from 20 to 25 feet in depth. 'The Yazoo Basin is sprinkled with towns whose sites were the home of the bear and the wild cat ten years ago.'

The engineers of this district in their reports of the flood of 1903 state that about one fourth of the Yazoo Basin was under water during this flood. Two crevasses occurred, letting water into the basin. One of these, three miles below Greenville, Mississippi, was at its greatest width a breach of 3,900 feet. The water flowed back upon Green-

ville and more than one half of that city was under water. Forces were set to work building a protection levee in the city and moving goods to a place of safety. As far as can be ascertained no lives were lost. Little or no loss in crops was sustained, as the flood came before the planting and the area was largely drained before that season arrived. The losses were mainly in live stock, fences and buildings. In contrast with the security so easily shown, in the reports of the commission, to be the experience of the inhabitants of the Yazoo Basin, the engineers in charge of the levees seem to congratulate themselves that no other crevasses occurred, for many weak places developed that required the utmost care and attention to hold intact. They state as their expectation that crevasses are liable to occur at any high-water season and at any point in the system or 'until all levees are brought up to a sufficient section to withstand the long-continued strain due to the water remaining for weeks near the top.' Although the statement quoted, on the face of it, rather begs the question, we are at liberty to infer therefrom that too much confidence had better not be held in the protective value of much of the present line. So near to disaster do the floods approach oftentimes, that every element which the engineers can control is considered a necessary ally in cooperation for the protection of the levees. During the 1903 flood, the river boats were required to run at a reduced speed along a portion of this basin front. So full was the river and the waters stood so near the top of the levee that it was not considered wise to subject the embankment to the wash of passing steamers. It may be stated in this connection that a storm, arising as the water is nearing the crest of the levees, can not be so summarily dealt with; and it often causes a day or two of apprehension, if indeed the beating waves do not tear their way through the structure.

A harsher note is sounded by an observer of the Weather Bureau[3] than is struck in the reports of the commission. There were favorable and mitigating circumstances which decreased the volume of the 1903 flood. Two factors materially modified the destructive feature of the flood; one of these was an early occurrence and the other a shorter duration. Both these factors spring from the same cause. Notwithstanding these compensating qualities this writer reports that the water during a period of two weeks was higher at Greenville and Arkansas City than it had ever before been known to be. The destructive work of the flood is summed up as follows: Some loss of stock in the basin; 115 houses evacuated in Greenville; 200 acres of fine farming land badly washed and left covered with sand; suspension of traffic on the Yazoo and Mississippi Valley Railroad from March 27 to April 17, and on the Riverside Division from March 27 to May 7; 1,460

[3] Bull. M, 'The Floods of the Spring of 1903 in the Mississippi Watershed,' H. C. Frankenfield.

square miles of land overflowed in this basin, one half of which was farm land; 60,000 people lived in the overflowed district and were, therefore, inconvenienced; this number of people represents about one third of the inhabitants of the basin. For some time previous to the coming of the flood, the dwellers in the basin were preparing for the flood season. Mounds were built for temporary refuge. Stationary platforms were constructed to the same end. Rafts were also made. The mules, horses and the feed were in many instances transferred to places of safety, often to the lofts of the barns. Farm implements and machinery were put beyond the reach of the water. That the warning of the Weather Bureau was so extensively heeded explains why there was no loss of life and little loss of stock.

These three reports of the same thing are not so contradictory as they sound. Each observer is looking for the things that sustain him and his point of view, and is not directly interested in the things that are foreign. One writer tries to establish the security of the basin of the Yazoo against danger to life and property, because that is what the board was created to do; a second writer tries to show how weak the levee is, in order to press home the need of funds—and he makes imminent danger to the basin area a means; the third shows that without the services of the branch he represents, the loss of property and life would be multiplied. The first man is right to some extent, and he is sustained by the second, who sees how near to each other danger and safety sometimes approach—and they are aided by the third. I doubt not but that the Weather Bureau may make as just a claim for the credit of the progress in the Yazoo Basin as the River Commission.

If the increasingly better reports influence a larger population and larger expenditures in holdings within the alluvial basin of the Mississippi, and the hopes of the engineers become realized to the extent of normal safety, then, perhaps, the levee system can be called efficient. Twenty years may be too short a time to consider the effect of the system upon population, and at the same time we must remember that but two thirds of the levee lines are completed, yet in this time the commissioners report an increase of population over the Yazoo Basin of over 100 per cent. It seems as if the people were becoming confident that there is 'security and permanence of protection' in the work that is in progress. Yet just so far as this confidence is expressed in settlement within the area liable to overflow, so much further must the levees protect beyond peradventure of disaster. In an increase of 100 per cent. in population and a decrease of 50 per cent. in mileage of overflow, if the terms are commensurate, there is no gain; if the terms are incommensurate, there is as good a chance for a loss as a gain. Just meeting the limit of strain, or preventing a break only by excessive vigilance and energy, or saving from disaster by some mitigating circumstance is not the end to be aimed at; but to be as reasonably sure as it is given man to be that an overflow can not occur must be the plan.

THE JEWS: A STUDY OF RACE AND ENVIRONMENT

By MAURICE FISHBERG

IN the search for the causes of various social phenomena characteristic of the Jews, most writers have been content to give 'race influence' a prominent place. The effects of the physical and social environment on the individual, or group of individuals, have been neglected. Once that remarkable cloak for our ignorance, 'race,' had served the purpose of explaining easily the causation of a given social fact, it was an easy matter to rest content with this explanation. It was repeatedly alleged that the Jews, though scattered in all the regions of the habitable globe, subjected to all varieties of climatic, social and economic conditions, nevertheless present everywhere the same characteristics with a remarkable uniformity. Demographic and social phenomena, such as fertility, mortality, marriage rates, illegitimacy, intermarriage, divorce, criminality, etc., were all attributed to ethnic origins, to Semitic influences.

Anthropological research has, however, revealed that there is no such thing as Jewish race, that ethnically Jews differ according to the country and even the province of the country in which they happen to live, just as catholics or protestants in various countries differ from each other. It was shown that there are various types of Jews, tall and short, blond and brunette, brachycephalic and dolichocephalic, etc.; and that all these types appear to correspond to the types encountered among the non-Jewish population among which they live. 'Race' can, consequently, not be the only cause of the demographic and social peculiarities said to be characteristic of the Jews. Other causes are to be sought for.

In the following studies statistical data of recent censuses in various European countries have been utilized in an attempt to find primarily whether the Jews do actually present uniformly, as has been alleged, similar social and demographic phenomena in every country, irrespective of difference of the physical and social environment. While the ethnic factor has not been neglected, still, in cases in which race influence is not sufficient to explain satisfactorily a social or demographic fact, or is in direct contradiction with actual conditions, the effects of the physical environment and of social conditions have been looked into. The author assumes that if an ethnic cause exclusively underlies a given social fact observed among the Jews, then we should

expect that the Jews in every country would present the same peculiarity.

I will begin with the question of the fertility of the Jews.

I. Natality

From the enormous mass of vital statistics collected during the past century, nothing definite could be established as to the influence of race on the birth rate. On the one hand, one would be led to believe that the Teutons have a high birth rate, when judged by the proportion of births in the German empire, but, on the other hand, in Scandinavia, where the Teutons have preserved themselves in a much greater purity, the rates are comparatively low, and the same is true of England. The Slavonic races in eastern Europe have a very high degree of fertility, but the differences in the various provinces of Russia, Poland and Austria are so great as to disprove directly the contention that race is necessarily the cause. In the same manner, the differences in the rates in Italy and southern France are striking. The racial elements are about the same in both countries, yet the birth rate of Italy is much higher than that of France. The Jews in Europe, owing to their isolation and alleged abstinence from intermarriage with other peoples, should offer good material for the solution of the question on the influence of race on fertilty.

Country.	Year.	Birth Rate per 1,000 Population.[1]	
		Jews.	Christians.
Algeria	1903	44.67	32.57
Galicia	1900	38.01	45.86
Warsaw (Poland)	1897	35.79	37.92
European Russia	1897	35.43	53.36
Austria	1901	33.89	38.01
Hungary	1900	33.81	39.34
Roumania	1902	32.36	42.86
Bukowina	1900	29.54	42.81
Amsterdam	1900	24.82	31.53
Lower Austria	1901	20.51	32.10
Prussia	1903	18.40	36.03
Bohemia	1900	17.85	34.88
Bavaria	1903	17.80	37.80
Berlin	1904	17.02	27.36
Prague	1901	15.85	31.31

It has been observed all over Europe that the birth rates of the Jews are low. When compared with the non-Jewish population of some countries, like Prussia, Bavaria, Bohemia, etc., they are only one half as fertile as the christians. Some authors have asserted that

[1] The term 'christian' includes: In Algeria, the Europeans living in that colony, the Mohammedan inhabitants being polygamous can not be compared with the monogamous Jews; in Warsaw Roman Catholics are referred to, and in European Russia, Greek Orthodox; in all the rest it includes the total non-Jewish population of the country.

this has a definite physiologic or ethnic basis as its cause, which is common to all Jews living in different countries. The social environment, such as economic prosperity, occupation, city life, etc., and also climatic conditions, were not considered. 'Race' was a satisfactory explanation. But the figures in the appended table giving birth rates of the Jews and christians in various European countries disprove the ethnic theory of the low birth rate of the Jews. If it was a physiological characteristic of the Jews, we should expect that the rates in every country would be about the same. As a matter of fact, however, the figures show wide limits of variation. In Algeria the rate is 44.67 per 1,000 population; in Galicia, 38.01; in Russia, 35.79, etc., while in Bavaria and Bohemia it is only 17.8, and in the city of Prague only 15.85. Ethnic conditions are never known to display such wide limits of variation.

It appears from these figures that wherever the rates are higher among the christian population, the Jews also show higher rates and the reverse. This is particularly striking when separate provinces of a country are considered. The following figures, taken from Ruppin's work on the Jews, illustrate this fact in Austria:

Province.	Year.	Birth Rate per 1,000 Population.	
		Jews.	Christians.
Bohemia	1900	17.85	34.88
Lower Austria	"	20.51	32.10
Bukowina	"	29.54	42.81
Galicia	"	38.01	45.09
Total Austria	"	33.89	38.01

It is thus seen that in Galicia and Bukowina, where the birth rates of the christians are high, the Jews also have a high rate, while in Bohemia and Lower Austria the rates for both Jews and christians are low. In the fifteen provinces of Russia which constitute the so-called 'Pale of Settlement,' the same phenomenon was revealed in the statistics collected during the census of 1897, the birth rate of the christian population being 51.71, and that of the Jews, 32.13 per 1,000 population. From the figures presented below, it appears that in the provinces where the higher birth rates are observed among the christians the Jews also are more fertile, and the reverse. Thus in Ekaterinoslav and Kieff the highest rates are recorded among both Jews and christians, while in Kovno, Bessarabia and Wilna the lowest rates are recorded both among the Jews and the christians. With two exceptions (Minsk and Poltava) the rule appears to hold good.

	Christians.	Jews.
Ekaterinoslav	61.82	42.28
Kieff	55.44	39.92
Minsk	54.90	29.76

	Christians.	Jews
Cherson	54.30	34.15
Volkynia	53.31	35.67
Taurida	52.51	34.48
Mohileff	52.30	28.16
Podolia	52.06	34.66
Grodno	50.96	29.84
Chernigoff	50.67	28.13
Poltava	49.00	36.51
Witebsk	46.85	31.00
Wilna	45.10	21.72
Bessarabia	44.46	27.91
Kovno	40.98	27.34

An attempt has been made by several statisticians to find some geographical differences in the birth rates of Europe. Sundbärg points out that some striking differences are to be noted in the rates when eastern Europe is compared with western Europe. He calculated an annual rate per 1,000 population for eastern Europe, 46.1; for western Europe, 33.6; southwest, only 32.3, and northwest, 34.7. On the whole, his calculations are well-founded, although there are some exceptions which are attributed to social conditions of a local nature. A glance at the table of the birth rates of the Jews in various European countries shows that while their fertility is everywhere lower than that of the christians, still in general they follow the rule laid down by Sundbärg. Taking Russia, Poland and Galicia as typical of eastern Europe, we find that the rates for the Jews are highest, reaching 38.01 in Galicia. Considering Bavaria as typical of the west, we find here the lowest rate, only 17. Amsterdam is intermediate between these two, only 24.82, corresponding roughly to the northwest of Europe. For the south there are no available data, except some collected in the middle of the last century (1861) showing that in Tuscany the birth rate was 27.2 among the Jews as against 39.0 among the christians.

It thus appears that the Jews follow quite closely the rates observed in Europe. The highest rates are observed in the east, the lowest in the west, etc. It is also known that in Denmark the birth rate of the Jews is very low, corresponding to the north, and in France conditions are similar to those observed among the French. In general it can be stated that with some local exceptions Sundbärg's rule holds as good for Jews as for non-Jews in Europe.

It would be misleading to explain the lower birth rates of the Jews when compared with christians as due to a physiological characteristic having as its cause a peculiar ethnic trait. The facts that the rates are not everywhere the same, but show wide variations, and that these variations correspond more or less closely to those observed among the non-Jewish population, are against any such theory. A close study of certain social conditions of the Jews offers a more reliable explanation.

It is noteworthy that the birth rates of the Jews are decreasing much more decidedly than those of the Christians in the countries in which they live. In Poland, for instance, the birth rate of the Jews was in 1891, 36.98, sinking in 1901 to 30.85, while among the Catholic population of that city it remained stationary, 41.58 and 41.59, respectively. In Roumania it decreased among the Jews from 40.14 in 1896 to 32.36 in 1902, as against an increase of from 41.19 to 42.86 among the Christians. In Hungary also the rate sank from 36.86 in 1891–95, to 32.19 in 1903. In western Europe this decrease of the birth rates among the Jews is actually appalling. In Bavaria the difference between 1876 and 1903 is nearly one half:

	Annual Birth Rate.	
	Jews.	Christians.
1876	34.4	45.9
1903	17.8	37.8

The birth rate of the Jews has thus decreased nearly one half within twenty-seven years, while among the christians the decrease is only slight. That this is not due to any special cause operating in 1903 is shown by the fact that it has been steadily going down. The average annual rates were in Bavaria as follows:

1876–1880	33.5
1888–1890	26.3
1890–1900	19.9
1903	17.8

In Prussia the same phenomenon is to be observed. The rates have decreased since 1875 among the Jews and increased among the Christians. As Arthur Ruppin shows, if we consider the absolute number of births during 1875 as 100, then it is found that during 1903 only 61.56 per cent. were recorded among the Jews, while among the christians it increased to 118.47 per cent. In the following table, giving the rates in Prussia for eighty consecutive years, is shown the steady fall of the fertility of the Jews; it shows that the christians bear nearly double the number of children as the Jews:

	1822-40	1841-66	1878-82	1888-92	1893-97	1898-02
Christians	40.01	39.55	37.92	37.03	36.89	36.19
Jews	35.46	34.75	29.96	23.75	21.61	19.71

The same is found to be the case in the United States: from statistics collected by the eleventh census (Census Bulletin No. 19, 1890), it is seen that the birth rate of the Jews is only 20.81 per 1,000 population, which is at least ten per 1,000 lower than the average birth rate among the general population. A fairer means of comparison,

however, is the ratio of births with reference to the number of women of child-bearing age present, *viz.*, those between 15 and 49 years of age, inclusive. This rate was found to be 72.87 per 1,000, as against 82.9 in Massachusetts, and 86 in Rhode Island. During the six years in which this investigation was made by the census officials, the rates among the Jews were decreasing perceptibly, showing the same tendency as is observed among the Jews in Europe. Physicians who practise their profession among the immigrant Jewish population of New York City all agree that its fertility is decidedly decreasing. Those who have been a longer time in the United States are always inquiring about the best means of limiting the size of the family, while the native Jews are hardly to be distinguished in this respect from the average American city population.

In no country in the civilized world is there to be seen such formidable decline in fertility as among the Jews in western Europe. In Germany the rates among the christians have remained about the same since the beginning of the nineteenth century; since 1840 the rates have been about 36, and remained so at the beginning of the twentieth century. In some provinces it has only slightly decreased, as in Prussia from 37.8 in 1841 to 36.5 in 1900. The most striking decline in procreative capacity is said to be observed in France. But even there it was 27.3 in 1841–50, and it sank to 22 in 1900. This is considered the most appalling decline. But among the Jews racial self-effacement is much more pronounced; in Prussia the birth rate sank from 35.46 in 1822–40 to 18.71 in 1904; in Bohemia, Bavaria, etc., the rates are lower, only 17 per 1,000 population; in large cities like Berlin, it is even lower, almost on the verge of reaching a vanishing point. The effects of this violent race suicide are evident to every one: the number of native Jews in those countries is decreasing in rapid strides to an extent unknown in the history of any civilized people.

It has been stated that, notwithstanding the low birth rate, the Jews have a higher marriage fecundity or fruitfulness than the christians. This is not borne out by facts. In the following table it is seen that in Russia, Prussia and Bavaria the average number of children to a marriage is smaller than among their non-Jewish neighbors.

Country.	Year.	Jews.	Christians.
Russian..	1897	4.33	5.63
Poland (Warsaw).....	1901	4.59	2.95
Austria.....	1901	5.37	4.59
Roumania.....	1902	3.22	2.15
Prussia	1875–99	3.77	4.40
Bavaria...	1903	2.31	4.30

That this low fecundity has not always been observed among the Jews is shown from figures about the Bavarian Jews collected by J. Thon.

The average number of children per marriage was during 1876–80, 4.75; it decreased during the next five-year period to 4.15; during 1886–90 to 3.49; then a further fall was observed to 3.01; during 1896–1900 it was only 2.50, and during 1902 and 1903 it again decreased, falling to an average of 2.20 and 2.31 children, respectively, per marriage.

In western Europe, where the birth rates of the Jews are lower, their fecundity is also lower. As will be seen later, this goes hand-in-hand with late marriges, celibacy, etc., among the Jews. In Russia, Poland, Galicia, Algiers, etc., where they are isolated from their christian neighbors and remain unaffected by what is generally known as modern civilized life, they marry earlier, have few celibates and raise large families. The birth rates are as a result quite high, though not as high as among the christians, who are largely engaged in agriculture and marry even earlier, as is the case in Russia and Galicia. Yet it must be remembered that in small cities in eastern Europe it is considered a sin for a Jew to remain unmarried, and an old maid in the family is a disgrace. In western Europe, on the other hand, the Jews are on a high social, economic and intellectual plane. Such people can not afford to marry early, and, after marriage, are not anxious to raise large families, for reasons known to-day in every large city. As a result they bear fewer children. Striking illustrations of this condition are presented in Austria. In Galicia and the Bukowina the rates are high, which goes hand-in-hand with poverty and strict adherence to their religious belief; in Bohemia and Lower Austria the rates are low, corresponding to the social and economic prosperity of the Jews in these provinces with the concomitant late marriages, celibacy, voluntary restriction, etc. In the United States also the newly arriving immigrants have a high fecundity, while the native Jews rarely raise large families.

Sex at Birth

The number of boys at birth exceeds the number of girls among most European nations. In some countries, like Greece and Roumania, the ratio is 112 to 100 girls, but the average appears to be about 103 and 105 for European countries. It was alleged that among the Jews this excess of male births is more pronounced than among the non-Jewish population. Ignorant, as we are, of the cause of the preponderance of males at birth, this excess, not being influenced by the social and physical environment, was considered a race trait of the Jews.

From statistics of the Russian Pale of Jewish settlement it is seen that there is actually a very large excess of male births among the

Jews: During 1897 there were recorded 115,344 Jewish births, of which 66,036 were males and 49,308 were females, or 133.91 boys to 100 girls. But a careful study of these figures brings forth strong suspicion as to their accuracy. Thus, when we examine the various provinces we find great variations. In Taurida the ratio was only 106.15 boys to 100 girls; in Cherson, 112.15; in Poltava, 112.87; while in Wilna it reached 177.47; in Grodno, 170.62; in Minsk, 165.45. In general, it can be stated that in the southern provinces the excess of males is not much larger among the Jewish population than among the christians, while in the northwestern provinces the excess is very high. That climatic conditions are not the cause is shown by the fact that among the christians the excess of males is not much more pronounced in the northwest than in the south. In Wilna it was only 110 and in Grodno 112. Two provinces not far distant from each other, like Wilna and Curland, show great differences in the proportion of male births among the Jews—172.8 in the former and only 115.4 in the latter. Climatic conditions can not therefore be considered.

If the excess of males were really as large as the above figures would seem to indicate, we should expect that the number of male infants below one year of age would also be excessive among the Jews. But from the census statistics of 1897 it is shown that it was only 104.21 boys to 100 girls below one year of age. The higher mortality of male infants is not sufficient to account for the loss of so many boys during the first year of their life.

The only plausible explanation for this apparent excess is that a large number of female births are not reported to the authorities by the midwives and rabbis, who are expected to register each birth. The birth of a boy in a Jewish family is accompanied by important festivals and ceremonials, while the birth of a girl, particularly among the poorer classes, is not considered of any special importance and is not attended by any ceremonials. It is very dangerous in later life for a boy who has not been registered at birth: he can not obtain a passport, and may be drawn into military service unjustly. All this brings it about that practically all the boys are registered, while a large number of female births is missing from the registry books. That this is the true explanation is seen from the fact that in 1893 the proportion of male births was 145.9 to 100 females, while in 1899 it was only 130.6, indicating a more complete registration of female births in recent years.

If the excessive proportion of male births was a racial trait of the Jews it would be expected that the same phenomenon should be observed among Jews in other countries. But this is not the case. In Warsaw, Poland, the ratio was in 1897 only 106 boys to 100 girls.

Ethnically there are hardly any differences between the Polish and Lithuanian Jews, still the latter show a ratio of 173 in Wilna, which again confirms the opinion that the excess in Wilna is due to neglect in reporting female births. In Prussia also the proportion was in 1893–1902, 106.24 (105.94 among the christians); in Austria in 1901 it was 107.85 (106.04 among the christians). In Prague the number of male births among the Jews in 1901 was equal to that of the female births, although among the christians there was an excess of males amounting to 104.1 per cent. In the United States the excess of male births is not large among the Jews, only 103.16 (Census Bulletin No. 19, 1890), while among the general population of Massachusetts and Rhode Island it is much higher.

Proportion of Stillbirths

Older statistics of stillbirths quoted by Bergman, Lagneau, Jacobs, etc., indicate that stillbirths occur less frequently among Jews than among Gentiles. More recent data on the subject show that this is not the case with the Jews in every country. Thus in Amsterdam the proportion of stillbirths in 1900 was among the Jews 3.48 per cent. of the total number of births, and much larger among the non-Jewish population, 4.81 per cent.; but in Warsaw it was in 1901 5.68 per cent. among the Jews, and only 4.13 among the christians. On the other hand, in Bavaria, it was in 1902–03 about the same among both, Jews (2.6 per cent.) and christians (2.9 per cent.). In Austria there are also no important differences. In 1901 the percentage of stillbirths was among Jews 2.61 and among christians 2.79. The most reliable statistics are collected in Prussia. The following are the percentages

	Jews.	Christians.
1875–1899	3.20	3.58
1901	3.07	3.03
1902	2.93	3.00
1903	2.83	2.99

There is practically no material difference in this respect among Jews and christians in Prussia. It should be mentioned in this connection that the smaller number of illegitimate births among the Jews would lead one to expect a smaller percentage of stillbirths, because the proportion of stillbirths is very large among illegitimates. The suggestion made by some that the large proportion of boys born among Jews is due to the fact that the percentage of stillbirths is small is also not to be seriously considered, simply because the proportion of stillbirths is not smaller among them. It must, however, not be overlooked that the percentage of stillbirths among the Jews varies with conditions observed among non-Jews in a given country. It is high

among the Jews in Warsaw, and low among the Jews in Prussia, just as it is among the christians in these countries. In other words, in eastern Europe, where childbirth is attended to by ignorant midwives, the proportion of stillbirths is larger than in western Europe, where either physicians or trained midwives are in attendance. Stillbirths are, after all, greatly dependent on economic conditions. They are very frequently met with among people in the lowest social and economic strata, and rare among the prosperous.

Illegitimate Births

Illegitimacy has often been taken as an index of the morality of a community. While it may be a true index in many countries, yet in some countries, owing to special marriage laws, an excessive proportion of illegitimate births is not necessarily an indication of vice. A good illustration is presented in Austria. There a child is considered illegitimate in case the parents have not registered their marriage with the civil authorities. It appears that the Jews in Galicia and Bukowina very often neglect to register their marriages and consider their religious ceremony as sufficient. As a result of this special law, it is found that while nowhere else is the proportion of illegitimate births among the Jews over four per cent., it reaches in Austria 61.37 per cent. In Storozynee the records even show 99.61 per cent. of illegitimate births among the Jews, which is manifestly absurd.

Country.	Year.	Per Cent. of Illegitimacy.	
		Jews.	Christians.
Bavaria	1876–1900	1.80	13.60
"	1902	2.39	12.40
"	1903	2.55	12.50
Amsterdam	1900	1.71	5.88
Austria	1901	1.37	11.38
Warsaw	1901	2.14	14.33
Prussia	1875–1899	2.83	8.81
"	1901	2.89	7.88
"	1902	3.90	7.13
"	1903	3.46	7.06
Berlin	1904	5.55	16.70
Budapest	1898–1902	12.50	30.50
Russia	1897	0.36	2.61
"	1898	0.37	2.67
"	1899	0.54	2.65

It is seen from the above figures that about seven illegitimate children are born to christians in Bavaria to one to Jews; in Amsterdam it is about three to one, in Warsaw seven to one, in Prussia and Budapest two to one, and in Russia five to one. The high percentage in Austria and in part of Budapest has already been explained above as being of no significance.

It is noteworthy that the percentage of illegitimacy among the Jews increases as we proceed from east to west of Europe. It is very low in Russia, about one-half of one per cent. higher in Bavaria, 2.5 per cent., and reaches over three per cent. in Prussia, while in Berlin it is even 5.55 per cent. This indicates that where the Jews are not affected by modern civilized conditions, the chastity of the women is much superior, the family ties are much stronger, and the girls only rarely go wrong. In the small towns of Russia, Poland and Galicia, one only rarely hears of a Jewish child born out of wedlock. Unmarried women seldom associate, even socially, with men before marriage. The absence of alcoholism, particularly among Jewesses who never drink, is another factor in keeping the sexes apart. But in the large cities in eastern Europe, where the separation of the sexes is not so strict, illegitimacy is encountered. In western Europe it is more frequent for the same reason. It was shown by Ruppin that in Germany illegitimacy is rarer among the Jews in eastern Prussia (Posen, Pomerania, East and West Prussia) where they adhere strictly to their orthodox religion, while in the large cities, where they have adopted many of the habits and customs of their christian neighbors, the percentage of illegitimacy is much higher, though still smaller than among non-Jews. In Russia also it is rare in Lithuania, only 0.02 per cent. in the province of Wilna, 0.24 per cent. in Minsk, 0.19 in Kovno, etc., while in the southern provinces it occurs more often, reaching 1.57 per cent. in Bessarabia and 1.19 per cent. in Ekaterinoslav.

It is well known that illegitimate births are very rare among women living with their parents, while agricultural servants, domestics, factory hands, etc., show the highest percentage of births out of wedlock. The Jewish women in eastern Europe only rarely live away from their parents or relatives, comparatively few are engaged in domestic service, and practically none are agricultural servants. In the small town a Jewish girl rarely works outside of her home. In western Europe social conditions of the Jews are nearer those of the christians among whom they live, and illegitimacy is more frequent than in the east. But inasmuch as the economic condition of the Jews in western Europe is superior to the average non-Jewish, the women being taken better care of, illegitimacy is rarer than among Gentiles.

THE DEVELOPMENT OF MECHANICS

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THE history of mechanics affords a notable instance of what may be called the relativity of science. In the course of its development three distinct sets of mechanical principles have been formulated, each having served in its turn as the foundation of a complete system of mechanics. The first set of principles may be regarded as the first mental image which man formed of the causes underlying the natural motion of material bodies, and, although admirable in many respects, was necessarily somewhat crude and naïve. With increased mentality came the formation of a new image, showing a greater maturity of thought than the first and offering a more powerful method of analysis. Finally, in recent times, a third image has been formed, which, although not essentially different in content from the others, exhibits a greater refinement of thought and expression. It is the purpose in what follows to outline briefly these three stages of development, and sketch the chief characteristics of each.

The first scientific development of mechanics arose from investigations concerning the equilibrium and motion of the simple machines in common use, such as the lever, inclined plane and pulley. This order of development was inevitable for the twofold reason that these implements had become familiar by centuries of use, and that they made a direct appeal to the understanding through the grosser and more elementary sensations of weight and pressure. In the second century, B.C., these investigations culminated in Archimedes's famous statement of the principle of the lever, but for seventeen centuries thereafter this statement remained the only instance of correct reasoning on natural phenomena. Apparently human experience did not yet suffice to extend the interpretation of natural law, as witnessed by the Ptolemaic system of astronomy, and Aristotle's division of motions into *natural* and *violent;* a classification which served rather to obscure than elucidate the subject.

In the latter part of the fifteenth century a fresh start was made, and the principle of the lever, handed down from Archimedes, was further investigated and generalized by Guido Ubaldi and Leonardo da Vinci. In 1586 these results were extended by Simon Stevin, who, by hanging a string of fourteen balls over a triangular support, established the properties of the inclined plane, and generalized his

results by stating the triangle of forces. These pioneers were followed by a host of lesser investigators, and by the middle of the sixteenth century this activity had resulted in the establishment of that branch of mechanics which is now called statics.

The next step was the introduction of the fundamental elements of time and mass in an attempt to investigate the laws of motion. At first little progress was made, as the misconception prevailed that a constant supply of force was necessary to keep a body in motion. Prolonged experiment and investigation, however, gradually resulted in a clearer understanding of these phenomena, and finally led to a correct statement of the first law of motion by the great Italian philosopher Galileo Galilei. Subsequent investigation of the motion of projectiles and falling bodies led Galileo to the two great ideas of inertia and the accelerating action of force, and enabled him to also state the second and third laws of motion. In addition to these great discoveries, Galileo generalized the law of equilibrium by stating the principle of virtual velocities, thus giving the first general solution of all problems in statics.

For the next century the development of mechanics consisted chiefly in an application of the principles of statics to liquids and gases. The only notable advance in mechanical principles during this period was made by Huyghens, who, in connection with his invention of the pendulum clock, investigated the center of oscillation and was thus led to a more general statement of the third law of motion.

The four fundamental ideas of space, time, force and mass were now firmly established, but until the time of Newton found expression only in an inorganic mass of facts and principles. Newton's discovery of gravitation, however, led to such a broad generalization of these ideas as to make possible a systematic treatment of the subject, and mechanics as a science may be said to date from the publication of his famous Principia in 1686. Newton's claim to preeminence, therefore, rests not on the discovery of new mechanical principles, but on the immeasurably greater service of bringing all natural phenomena under the reign of universal law.

Only one element was now lacking to complete the series of independent fundamental statements necessary to constitute the foundation of a complete system of mechanics. There still remained the establishment of a general relation between these fundamental concepts, and after eighty years of experiment and investigation along the lines indicated by Newton, this relation was furnished by d'Alembert in the statement of his famous principle.

This closed the first stage of development. The image was now complete, and henceforth a system of mechanics based on this foundation must be a purely deductive science. The subsequent history of

mechanics verifies this statement, for since the time of d'Alembert no essentially new principle has been discovered, and Gauss may be quoted as authority for saying that none ever can be.

The second stage of development was characterized by the elaboration of the system of mechanics formulated by Archimedes, Galileo, Newton and d'Alembert. In the course of this process a new view of the fundamental ideas underlying the subject was attained, which resulted in establishing mechanics upon an entirely different basis. The first step in this direction was made by Euler, and consisted in replacing the geometrical methods of Newton and his predecessors by those of analysis. Euler thus laid the foundation for a system of analytical mechanics which was brought to its perfection by Lagrange in his generalized equations of motion.

This new representation of mechanics was followed by the establishment in the early part of the last century of two great principles; the principle of least action and the principle of the conservation of energy. It is important to note in this connection, however, that each of these principles is deducible from that of d'Alembert, and, consequently, that their establishment did not increase the number of independent fundamental postulates.

The first of these principles dates back to the attempt of Maupertuis to establish on theological grounds a principle of similar nature but of much more limited scope. This attempt, although fruitless in itself, served to direct thought in a new channel, and finally led Gauss to the statement of his 'Principle of Least Constraint.' This in turn led investigators to the idea that all natural phenomena present a maximum or a minimum, and induced Euler and Jacobi to seek expressions whose conditions for a minimum would give the equations of motion. From this it was but a step to the establishment of Hamilton's principle, which consists in the analytical statement that the variations of work and energy vanish for the initial and final configurations. As Hamilton's principle includes both conservative and non-conservative systems, it constitutes a generalization of the principle of least action.

This second principle, like the first, was the product of evolution, as the ideas underlying it had been the subject of investigation from the time of Leibnitz and Descartes. The principle did not assume definite form, however, until the middle of the nineteenth century, when it was stated by several investigators almost simultaneously as the law of the conservation of energy. The names most closely associated with this principle are those of Mayer, Joule and Helmholtz, and it is curious to note that each of these scientists arrived at his results by a different process; Mayer by philosophical reasoning, Joule by experimentation and Helmholtz by mathematical analysis.

The establishment of this law marked the close of the second stage

of development. Energy replaced force as a fundamental idea, and a new system of mechanics resulted, founded on the relations between space, time, mass and energy, as embodied in Hamilton's principle.

Although a comparatively short time has elapsed since the establishment of energetics as the basis of mechanics, a third stage of development is already clearly marked. To characterize each stage by a single word, the first may be called *constructive*, the second *deductive* and the third, or present stage, *critical*. To the founders of the first two systems the concepts of force and mass, although more artificial than the intuitive ideas of space and time, were probably no less axiomatic. With the growth of modern scientific criticism, however, came the desire to go back of intuition, if that be possible, and subject the foundations of science to the last analysis. As the result of this tendency the foundations of the first two systems were found open to certain objections, which have been admirably expressed by the late Heinrich Hertz. The chief objection to the first system is in relation to the idea of force, any definition of which seems to involve its author in certain logical difficulties somewhat similar to those encountered in attempting to define a straight line. In the second system, criticism is aimed not at the fundamental concepts, but at the relation between them as expressed in Hamilton's principle, the objections to which are twofold: namely, that it has no simple, natural interpretation, and that it seems to endow matter with the attributes of thought and volition. A further objection is made to both systems on the ground of a certain redundancy in the fundamental ideas, three fundamental concepts being both necessary and sufficient, according to Kirchhoff, for the development of a complete system of mechanics.

In view of these and other objections, Hertz and his followers have outlined an ideal system of mechanics based upon three elements only: namely, space, time and mass. To supplement the deficiency caused by the lack of a fourth element without increasing the number of fundamental concepts, Hertz has introduced the idea of concealed motions acting in connection with those visible to the senses. This idea was originated by Lord Kelvin in his theory of vortex atoms, and was further developed by Maxwell in his attempt to explain electromagnetic action. The first complete treatment of concealed motions, however, was given by Helmholtz, and in the hands of his pupil Hertz it has proved a powerful instrument in establishing mechanics upon a more satisfactory basis.

What the future of mechanics may be it is of course impossible to predict. However, the brief review of its development that has just been given suggests that the foundations have reached bed rock, and that future effort must be directed toward the enlargement of the superstructure and its adaptation to the growing needs of humanity.

DIAMONDS AND CARBONS IN BRAZIL

By H. W. FURNISS,
FORMERLY AMERICAN CONSUL AT BAHIA

THE best diamonds of the world, those of finest color and fire, come from Brazil, though most of the stones mined are small in size as compared with those from other sections. They occur in various places, more particularly Goyaz, Matto Grosso, Minas Geraes and Bahia. It is only in the latter two states in which they have been found in sufficient quantities to warrant mining.

The exact date of the discovery of diamonds in Brazil is unknown. At the end of the seventeenth century miners were taking out gold in Minas Geraes at Serro Frio and failed to recognize the diamonds which were occurring therewith. It is said that some stones were collected more because of their regularity and beauty of crystallization than from knowledge of their value. These in 1729 found their way to Portugal, where they were recognized as diamonds of unusual purity. The discovery caused quite a sensation at the court of King John V. There was a rush to the newly-discovered region, but the king so restricted mining that little was done until subsequent to 1832, when the present laws became effective.

Diamonds were first discovered in Bahia in 1840 at Santo Ignacio at the extreme northwest of the present region, but not until 1844, when discovery was made by a slave on the banks of the Mocojé river, the present location of São João do Paraguassú, was any great impetus given to mining. The mining area has gradually extended, but no new section was discovered until 1881 when by accident a find was made at Salobro, the diamonds of which usually take the name of Cannavieiras, the port to which they pay tribute.

The quality of Brazilian diamonds varies greatly with the locality in which found, while there is always a considerable difference between those of the same mine. In general, those from Minas Geraes are fairly assorted in quality, about Salobro (Cannavieiras) the beautiful whites and priceless blue whites predominate, while the other Bahia stones are inclined to be more off-colored and frequently contain black specks, thereby lowering their value.

The greater part of the Bahia diamonds differ from those found elsewhere in the world in that they frequently have a thin coat of surface color which gives the whole stone a bad appearance. This color will not yield to acids. To one particularly skilled, the under-

lying true color can be determined, but to make the stones marketable abroad where this is unknown, recourse is had to heating them red hot and pouring on a chemical when the crust is consumed and the real color appears. I have seen apparently dirty red, green, brown, blackish and yellow stones after burning turn out to be pure whites and blue whites. Stones so treated lose in weight about one per cent., and those with cracks or defects frequently break to pieces.

The largest authentic Brazilian diamond ever found is the famous 'Estrella do Sul' (Star of the South). It weighed 254.5 carats in the rough, and cut and polished weighs 124½ carats, with a value of $450,000. The greater number of diamonds found are less than one carat; the average weight is about two carats, while a stone of 10 carats is a great exception.

Carbons which occur along with diamonds are very ordinary looking stones and would be refused as a present by any one not well acquainted with them. Their history is very obscure. Other than a few small ones found in Minas Geraes, and those are of poor quality, Bahia is the only known place where they occur. They seem to have been known in 1848, when a Frenchman traveling through Bahia bought them for twenty-seven cents a carat under the name of 'ferragens' (iron stones). In March, 1856, Mr. Domingos Gomez, of Roncador, took to London 6,475 carats, which he had bought for sixty cents a carat, and was more than pleased to sell them at $1.25 a carat. At that time their sole use was to be pounded to dust for use in diamond polishing.

The later history of the carbon is the history of the so-called diamond drill which now constitutes their principal use. For this purpose stones weighing from 1½ to 4 carats are desired and larger stones have to be broken to these sizes. The drill consists of 6 or 8 carbons set in a crown or cylinder of steel forming the bit. They are set in such a way that they alternately slightly project beyond the inner and outer edge, thereby cutting as they are rotated a core, which is brought to the surface from time to time as desired. Being the hardest known material they will cut the most refractory ores or stones.

As the drill goes around the carbons wear off and have to be from time to time reset, until finally they become so small as to be useless. For this reason, unlike the diamond whose chief use is for adornment, the number of carbons is constantly growing less while the demand is exceeding production. With the perfection of the drill and its great use in cutting tunnels, mines, canals, etc., the price of carbon has steadily gone up from $17 a carat in 1892 to $60 to-day in New York for the best quality of proper size, and the price obtained at the mines has been a fair equivalent.

The average weight of carbons encountered is much larger than

that of the largest diamonds. The frequency of occurrence where the two occur together is in the proportion of three of the carbon to one of the diamond, by weight, while good quality carbon is worth more per carat than gem rough diamonds. The miners know this, and it is their constant desire to encounter carbons. Many of them go to the sacred tree near Lençõe, where the spirit of Santa Barbara, the patron saint of the miner, is said to have appeared, and place on a limb thereof a stone of the size of the carbon they wish to find. They then pray and it is said that the saint has blessed many of the faithful.

The largest carbon ever encountered was found near Lençõe in 1895, on the ledge of a mountain which had been worked some time before. It weighed, when found, 3,165 carats, was purchased from the miner for $16,000 and was finally exported to London, where it sold for $31,145, having lost about 50 carats meantime in drying out. In London it was broken into pieces suitable for drills and these pieces sold for about $40,000, while at the present price of carbon they would be worth about $158,000.

The next largest carbon was found this year, and is still in the hands of the miner. It weighed when found 869 carats, but has lost

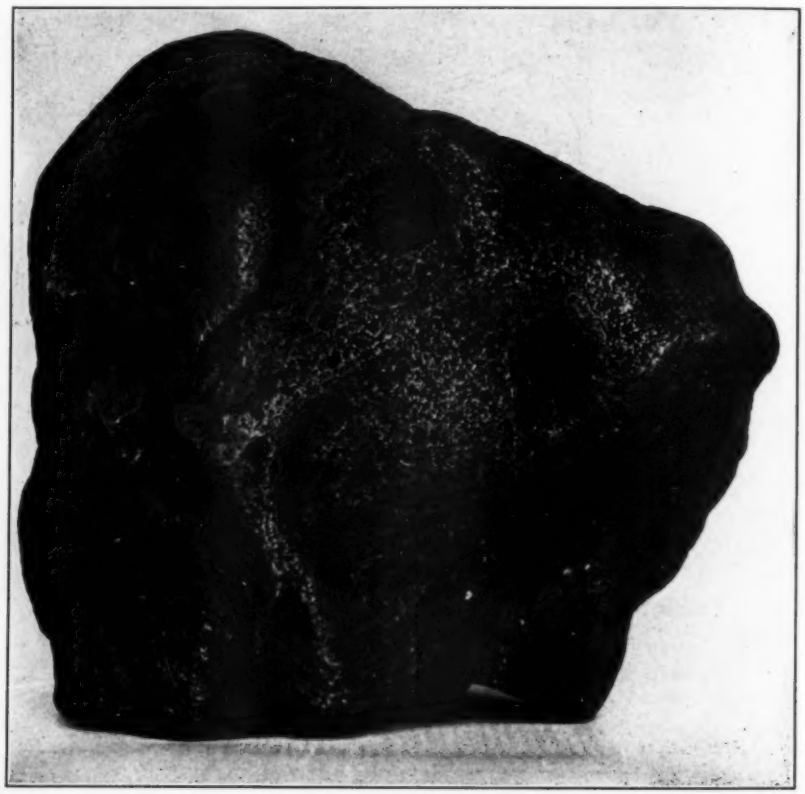


FIG. 1. THE LARGEST CARBON EVER FOUND.

several carats in drying. It is of finest quality and almost rectangular shape. The equivalent of $26,400 was offered for it six months ago, but the owner has set a price which to-day is the equivalent of $45,625, an impossible figure, as in breaking there is always considerable loss. When carbon advances beyond a certain figure the sale of necessity decreases, as then there are other products which are used even though lacking in durability and other desirable qualities.

The genesis of the diamond and carbon has not been worked out for this section. Whatever it proves to be, it is certain that at one time they were all confined in a conglomerate which shows evidence of being of more recent geological date. The conglomerate differs in character in the different sections. In the neighborhood of Lavras Diamantinas it consists of many colored water-washed pebbles and boulders, chiefly sandstone of the same nature as the strata found immediately below it; in the Salobro region it consists chiefly of granite pebbles. In both instances the matrix is sand of different degrees of hardness, fineness and color.

With the ages a great part of the conglomerate has disintegrated and the rains and rivers have washed the diamonds and carbons to the places where they are now being found. There are large masses of conglomerate in many places which have resisted this action, and unless mechanical means are brought to bear will continue to yield diamonds and carbons for the ages during their disintegration.

The region about Salobro is comparatively flat, in fact the greatest deposit occurs in an area practically level, doubtless the old river bed. In the other sections of Bahia the country is rocky and mountainous. There is so much of rock and so little of soil that only small plants grow, and then only during the rain time. In some cases the rivers pass through gorges cut into the solid rock and most precipitous and awe-inspiring. On all sides there is much of interest. The rock formation is a very hard reddish sandstone which completely underlies the conglomerate and like it shows the disintegrating effects of water and climate. In places it has deep cracks which have become natural canals, accumulating with the ages a concentrated diamond- and carbon-bearing gravel. In other places immense pieces of sandstone and conglomerate are piled up heterogeneously as if they had been dumped there. The canal Simplicio Braga is a combination of these two varieties and was one of the richest finds of the region.

The diamond section of Bahia is much more accessible than that of Minas Geraes. One can arrive at Andarahy, the heart of the region, in four days from Bahia City, five hours of one day being spent in journey by boat, twelve hours of the next by train and two days by mule. The trip is without hardships to one accustomed to travel. It is along attractive scenery, across rivers and mountains, passing through a section with beautiful calcareous caves, but with an entire

FIG. 2. HOME OF A WEALTHY MINE OWNER.

lack of water, except that of the river miles away and that caught during the few rainfalls.

Most of the mining is done by individuals called 'garimpeiros,' who either work for themselves or on shares with the owner of the claim. In Bahia, the number of owners hiring laborers to work their claims is not more than half a dozen.

The miners are almost entirely blacks or of mixed race. The greater part of them live in near-by towns, but many have quarters built beneath an overhanging ledge, from which they have removed the diamond-bearing material.

Their food consists chiefly of native beans with jerked beef and an abundance of mandioca meal, which takes the place of bread, with now and then fresh meat, a much prized boiled dinner or a piece of salt fish. Drinking water is in abundance everywhere. Native rum can be had very cheap, yet the number addicted to intemperance is very small, wonderfully so for a mining region.

Many times provisions are advanced by the grocer until a find is made, when all is paid up, and if there is a balance such high-priced articles as beer, American canned oysters, lobsters, etc., are indulged in as long as the money lasts.

The health of the region leaves much to be desired. Because of the great quantities of semi-stagnant water on every hand, every facility is given to create anopheles mosquitoes, with the result that malaria in its worst types is always in abundance.

By far the greater part of the successful mining is still done by antiquated methods which have the advantage that they require little capital for an outfit. A miner's tools consist of a short-handled hoe

with which to stir up diamond-bearing gravel in a sluice; a crowbar to pry up stone to lay bare deeper layers or to break down banks of clay or gravel; an iron hook on a pole with which to take diamond-bearing gravel from beneath large stones or from cracks otherwise inaccessible; a small wooden basin, called 'carimbé,' for carrying the gravel on the head; a large wooden basin, called 'bateia,' for final washing and concentrating the gravel; some kind of a sieve, from a tin can with nail holes to a more pretentious wire sieve, for sorting gravel and sand during the washing or concentrating process; a hammer and drill for making holes in rock for blasting, but quite often instead fire is built upon a rock desired to be removed, and after the

FIG. 3. HOME OF MINER, BUILT UNDER LEDGE FROM WHICH DIAMONDS AND CARBONS HAVE BEEN REMOVED.

rock has become very hot cold water is poured thereon, effectively cracking it and permitting its removal.

In the home of the carbon there are no carbon or other mechanical drills. At present one man can make from two to three holes a day, which with proper methods could be made in a few minutes.

The method of mining differs in various sections. In the richest areas the work is of two kinds: removing the subsoil surface disintegration and gravel and that in the gullies, cracks and beneath the more accessible stones, or mining by tunnels or following cracks into the pockets of the mountains, taking out the diamond- and carbon-bearing material consisting of soil, sand, gravel, boulders, broken and disintegrated stone, etc., called 'cascalho.'

The other method is in diving to the bottom of rivers and taking out the cascalho from there. This method is confined to a small section of the district where the river runs through a natural canal

FIG. 4. METHOD OF MINING; REMOVING SUBSOIL TO EXPOSE CASCALHO.

cut into the rock. The diving can only be done when the river is low and is chiefly done naked, though there are a few diving suits in use. The naked divers descend a pole planted in the river and fill a sack with the cascalho, which is taken on shore for washing. The ability of some of these men to go to great depths and stay under for long intervals is extraordinary. In some places attempt is made to work the old river bed, but this is done with great difficulty, as water will seep in almost as fast it can be bailed out, leaving little time for the collection of cascalho.

Whatever the method of taking the cascalho out, the great desideratum is an abundant supply of water for washing. Where it is possible water from mountain streams is conducted down by ditches and flumes, and into these the cascalho is thrown. It is worked with a hoe, by which method the lighter particles are washed away, thus leaving a greater concentration which includes the diamonds and carbons. The concentration is taken out of the ditches and accumulated until the week's end, when it is laboriously further concentrated in bateias.

This final concentration and wash-up requires considerable dexterity as well as strength. It consists in revolving and shaking the bowl that the portions of heavier specific gravity accumulate in the

point in the bottom, while the lighter particles and the large stones are thrown on the edge of the bowl and are from time to time scraped away with the hand, being examined meantime. While the vision of

FIG. 5. SIX MINERS BAILING WATER THAT ONE MAY COLLECT CASCALHO.

FIG. 6. FINAL CONCENTRATION AND WASH-UP IN BATEIAS.

those engaged in this process is very sharp and they will frequently yet from investigation I know that by this method many large dia-

take out from sand and pebbles diamonds smaller than a pin's head, monds and carbons escape them. This in part accounts for the reason why large diamonds and carbons are frequently found in gravel already washed and picked over. I have heard of places which have been washed for the fourth time and paid, though doubtless in some of these instances the later finds were due to disintegration of conglomerate which yielded up stones heretofore inaccessible.

The limit of a good man is to concentrate and pick over a cubic yard of cascalho per day, but this presupposes that the cascalho is easy of access and that the water is near at hand. If the cascalho has to be taken from the cracks, crevices, caves, etc., and with the present methods of mining those are the only places with virgin material which are accessible, it is accumulated very slowly. When it is remembered that at the South Africa mines there is worked over 192,000 cubic feet per day, it can readily be seen why the output of Brazil with its few thousand of hand-workers sinks into insignificance, if indeed the diamonds are in Brazil to extract.

The mines of Minas Geraes have been worked regularly since their discovery, chiefly by hand methods until during the last ten years when some machinery has been installed to aid in the separation of the diamond-producing gravel from the clay and sand and later on in partly sorting the gravel prior to the final clean-up which is always by hand process. In Bahia a little machinery consisting of a few pumps, a gravel sorter and a so-called automatic separator, which does not separate, has been installed at Salobro, but it is being allowed to rust out, work at present being done by hand entirely ignoring the machinery. The only other machinery in the great Bahia district consists of a few pumps mounted by an English company on the São José river. This company has machinery in transit for mounting a small electro-hydraulic plant, but will still leave the clean-up to hand process instead of adopting the automatic table in use at South Africa.

The diamondiferous lands of Bahia are owned by the state and leased either as small claims or large parcels to parties or companies desiring to work them. About all of the known areas capable of work with groups without machinery have been preempted. The nature of the work already done has been such that many productive areas have been covered with tailings. The river beds and other productive sections which will necessitate machinery are still awaiting exploitation.

SHORTER ARTICLES

SIGMA XI

Conspicuous among the events that attended the recent Ithaca meeting of the American Association for the Advancement of Science, was the twentieth celebration of the founding of the Sigma Xi, and, as so little is known about this organization, I venture to give a brief description of its history.

The career of the Phi Beta Kappa Society has been a long and honorable one, having been founded at William and Mary College in Williamsburg, Va., on December 5, 1776, and it is, therefore, the oldest of the so-called Greek letter societies. This organization, as is well known, admits to membership honor students in the humanities who are about to graduate, and the lack of any organization that should similarly recognize distinction in the study of the scientific branches led in 1886 to the organization in Ithaca of the Society of the Sigma Xi, which has as its objects to encourage original investigation in science, pure and applied, and by meeting for the discussion of scientific subjects, as well as for the publication of such scientific material as might be deemed desirable; and also to establish fraternal relations among investigators in scientific centers. Its name is derived from its motto Σπουδῶν Ξυνῶνες, signifying Companions in Zealous Research.

The success of the organization led to the establishment of a chapter in the Rensselaer Polytechnic in Troy and in Union University in Schenectady a year later. A chapter in the University of Kansas in 1890 and one at Yale University in 1895 followed. In 1896 a chapter was established at the University of Minnesota and one at the University of Nebraska in 1897. The Ohio State University came next in 1898, and the University of Pennsylvania in 1899. With the opening of the new century came chapters at Brown and the University of Iowa, and then Stanford University and the University of California, and Columbia University in 1901 and 1902. Three chapters were established in 1903, namely, at the University of Michigan, the University of Illinois, and the University of Chicago, and a year later organizations were effected at the Case School and in the University of Indiana.

Application for a chapter is now before the council for the University of Wisconsin. Thus it will be seen that this organization has already secured a good foothold and has been established at nearly all of the larger universities.

The first president was Henry S. Williams, of Cornell, one of the founders, who was succeeded by S. W. Williston, of the University of Chicago, who two years ago gave place to E. L. Nichols, of Cornell. Biennial conventions are usually held in connection with the meeting of the American Association for the Advancement of Science, the next of which will be held in December, 1906, many of its members being prominently connected with that organization. The membership is already a large one, numbering more than a thousand persons, most of whom are either teachers of or advanced students of science.

The different chapters hold public meetings at which speakers of eminence are invited to address the organization.

The badge or insignia is a watch charm or pendant consisting of the monogram formed in gold of the Greek letter Sigma superimposed on the greek letter Xi, the former being somwhat smaller than the latter. On the reverse side of the badge is engraved on the upper

bar the name of the college in which the owner was initiated, together with the date of such initiation; while on the lower bar is the name of the owner with the numerals of the class in which he was graduated. The society has adopted as its colors electric blue and white. Its seal consists of a wreath of laurel, typifying the honorary character of membership in the society, arranged as an oval enclosing the words 'the Society of the Sigma Xi' at the top and the Greek motto at the bottom. These words form an inner oval concentric with the first, punctuated with ten stars enclosing a field illuminated by the lamp of research. Above the lamp in the field of illumination is placed the monogram composed of the two Greek letters Sigma and Xi, and below it the date of the foundation, 1856.

At the celebration of the twentieth anniversary, representatives of nearly every chapter were present, and under the auspices of the local chapter a public address, commemorative of the occasion, on 'The Recent California Earthquake' was delivered by Professor John C. Branner, of Stanford University. Subsequently a dinner was tendered to the visiting members which was presided over by Professor E. L. Nichols, when addresses were made by Dr. L. O. Howard, who spoke of the affiliation of the Sigma Xi with the American Association, and by Professor Henry S. Williams who described its founding, and by other members of the society. M. B.

THE PROGRESS OF SCIENCE

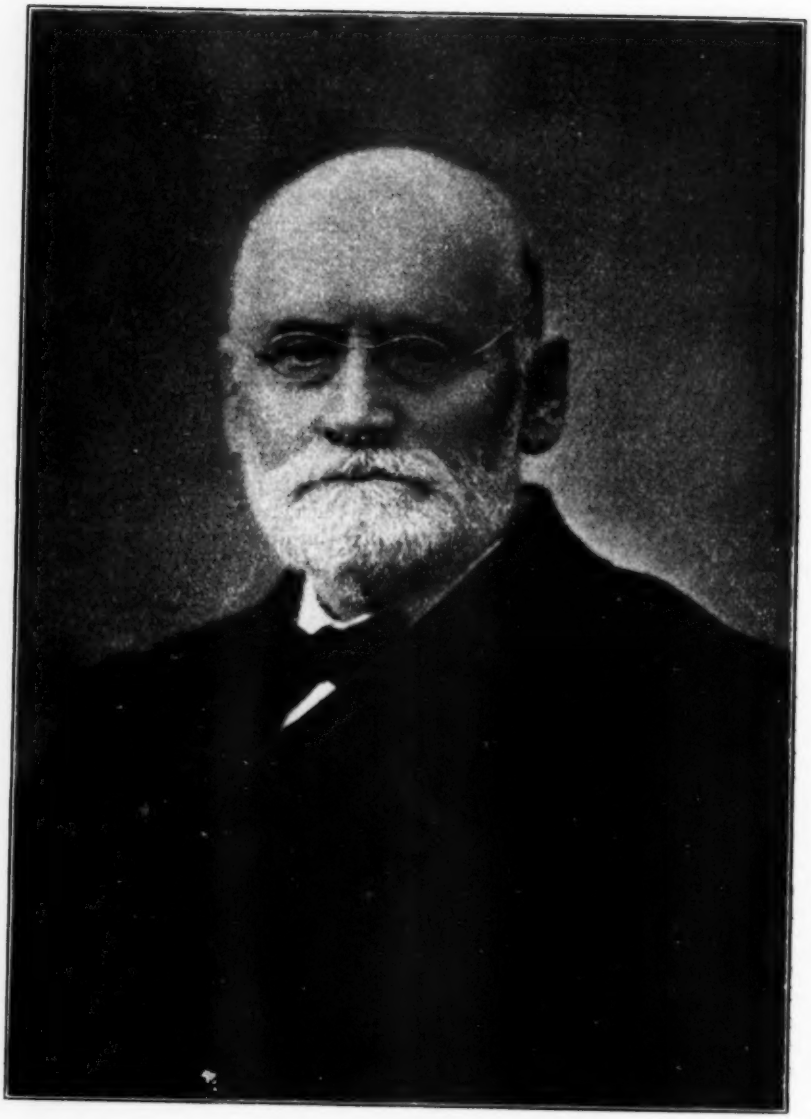
THE BUREAU OF EDUCATION.

THE retiring commissioner of education has been so completely identified with the Bureau of Education that it is difficult to imagine the institution without the man. Dr. Eliot, at Harvard, and Dr. Harris, at Washington, have been our two great educational leaders, and when we turn to other lines of service—to the church, to medicine, to law, to journalism, to business, to politics—it is doubtful whether we can find elsewhere two men equally great. This is not a time to attempt an analysis of the work and limitations of a complex personality. It is better to quote the appreciation of a personal friend, Dr. Canfield: "He is indeed whole in himself, a common good —a man of amplest influence yet clearest of ambitious crime, our greatest yet with least pretense; rich in a saving common sense, and, as the greatest only are, in his simplicity sublime. His is the good gray head which all men know, and his the voice from which their omens all men draw. In the great battle of the public schools for sound and effective citizenship he is a tower of strength which stands foursquare to all the winds that blow."

The commissionership of education has been filled by the appointment of Dr. Elmer E. Brown, professor of education in the University of California. We may again quote, this time from the editorial pages of the *Outlook:* "He has shown himself to be safe and sane, philosophic in temper, practical in choice of ends and means, with unusual administrative ability, ready to take the initiative, not carried away by undue enthusiasm for novelties, yet always alert for all that marks true advancement, energetic and active and industrious, an able writer and speaker, and of a personality which makes him very acceptable in the educational world. In many ways and because of many characteristics and qualities he promises to be a worthy successor of one of the most widely revered educators this country has ever had the good fortune to enlist in its service."

No one can fill the vacancy left by Dr. Harris, but the new commissioner has a great opportunity for useful service. It is safe to say that there is no other country where public education is such an important factor in national life and at the same time no other country in which it is so completely neglected by the national government. This paradox is of course due to the fact that public education is left to state and local authorities, as was doubtless intended by the federal constitution. But wisely or otherwise, the national government has continually extended its functions. If it can examine banks, it can examine schools; if it can cooperate with states in their geological surveys, it can cooperate with them in their educational systems. As a matter of fact the constitution gives the congress power to 'provide for the common defense and general welfare of the United States.' Under the changed conditions of modern civilization, education, science, health and well-being are far more important for the common defense and general welfare of the nation than are the army and the navy.

But apart from cooperation with the states, such as now in fact exists in the case of the Department of Agriculture and the land grant colleges of agriculture and the mechanic arts, there is ample room to strengthen the Bureau of Education. After a secretary of



DR. WILLIAM T. HARRIS

COMMISSIONER OF EDUCATION, 1889–1906

commerce and labor has been added to the cabinet, it would be only decent to provide for a secretary of education. There is probably no other nation without a department of education. The salary of the commissioner of education is $3,500, and the powers of his office are very limited. The bureau has charge of education in Alaska and prepares an annual report containing statistics and papers on education; but this is all. It may be wise to let the work of the government for education, science and art be distributed among different departments on the financial side. But there should be cooperation and a great extension of what is now being done. The Bureau of Education is the natural center, and we may look to a great enlargement of its powers and influence in the near future.

PROFESSOR MENDELÉEFF.

DIMITRI MENDELÉEFF, the greatest of Russian chemists, was born in Siberia seventy-two years ago. From 1856 till 1859 he was an instructor at the University of St. Petersburg. After two years of study at Heidelberg, he returned to Russia in 1861. Two years later he was made professor of chemistry at the Technological Institute in St. Petersburg and was transferred to the university in 1866.

From the beginning Mendeléeff has been interested in theoretical problems. His first paper was on isomorphism. For years he worked on the relations between specific volumes and other properties. While others, notably Kopp, have worked along similar lines without making any great generalization in consequence, it must be admitted that Mendeléeff's great discovery of the periodic law seems a natural development from the earlier work.

In 1869 Mendeléeff announced that if the elements be arranged in the order of their atomic weights, it will be found that similar variations in their chemical properties repeat themselves periodically, and that the order of the faculty of the elements to combine with other elements also corresponds with the order of their atomic weights.

Like many another important generalization, the real significance of this one is not self-evident. Before the periodic law was formulated, the atomic weights of the elements were purely empirical numbers, and it was not always easy to tell what multiple of a given value should be taken as the true atomic weight. This was changed by Mendeléeff's discovery. The periodic law made it possible to determine the atomic weights of yttrium, indium and beryllium, for instance. Mendeléeff went further than this. He pointed out that there were gaps in the table; that these must correspond to unknown elements; and that the properties of these unknown elements could be predicted from those of the known elements surrounding the gaps in the table. Gallium, scandium and germanium have since been discovered and have the properties assigned to them in advance by Mendeléeff.

A more striking, though less dramatic, proof of the soundness of Mendeléeff's generalization is to be found in the fact that the inert gases of the atmosphere, argon, helium, neon, etc., find places in the classification, though the possibility of there being such substances was not suspected in 1869. It is not too much to say that the periodic law of Mendeléeff is recognized to-day as the only basis for the classification of the elements. Only two contradictions have been found in nearly forty years. The atomic weights of the elements, iodine and tellurium, should be transposed to make these substances fit into the table, and there is no place for most of the so-called rare elements. The first difficulty will disappear if any one can show that either tellurium or iodine contains an unknown impurity. It must be admitted that the chances of this are not good at present.

We can avoid the difficulty as to the rare earths by considering a group of

PROFESSOR DIMITRI MENDELÉEFF

them as equivalent to one element. Doing this puts the rare earth elements on a somewhat different footing from the other elements. While this is justified to a certain extent by the chemical properties, it can not, in the nature of things, be a final solution. If we are not to throw over the periodic law, we must either split other so-called elements into groups of elements or we must show that certain groups of elements alone are possible. To succeed in the first would be to revolutionize chemistry. To succeed in the second would be to explain the reason for the periodic law—which would also revolutionize chemistry. Whatever the outcome, Mendeléeff's law will be for many years one of the dominant factors in chemical progress.

APPROPRIATIONS FOR THE DEPARTMENT OF AGRICULTURE.

THE agricultural appropriation bill for the fiscal year ending June 30, 1907, as finally passed by the recent session of congress, carries an appropriation of $9,932,940. Of this amount the sums appropriated for what may be termed work in applied science are distributed as follows: The Bureau of Animal Industry receives $4,029,460, but of this amount $3,000,000 are to be devoted to the meat inspection, the discussion of which has occupied so much of the time of congress and of the public press during the past few weeks; Weather Bureau, $1,439,240; Bureau of Plant Industry, $1,024,740; Forest Service, $1,017,500; Agricultural Experiment Stations, including the Department Office of Experiment Stations, $974,860; Bureau of Entomology, $262,100; Division of Publications, $248,520; Bureau of Soils, $221,460; Bureau of Statistics, $210,560; Bureau of Chemistry, $174,180; Office of Public Roads, $70,000; Bureau of Biological Survey, $52,000; Library, $25,880.

Large as is the annual appropriation

for the Department of Agriculture, it is only one six-hundredth of the value of the agricultural products of the country, and there is every reason to suppose that it is a good investment. The figures of our agricultural wealth as given in the last report of the Secretary of Agriculture are so vast as to be difficult to grasp. Thus the corn crop alone is valued at $1,216,000,000. Hay, cotton, wheat, butter and milk and poultry and eggs each produced products worth over $500,000,000. Farm products of the value of $827,000,000 were exported. Thanks to such exports the balance of trade in favor of this country in the course of the past sixteen years amounts to over $5,000,000,000. The farms of the United States are said to have increased in value to the amount of $6,131,000,000 in the course of the past five years.

The Secretary of Agriculture awards mainly to the department credit for the great advances in the prosperity of the farmer in recent years. In concluding his report he says: "The gratifying evidences of well-being in our farming community, the extraordinary progress made in the past few years, and the rapidly enlarging recognition of the true position of the farming industry in the economic life of this country are mainly the result of this continued and combined effort on the part of these agencies to add to the sum of the farmer's knowledge, and must be regarded as the triumph of intelligence in the application of scientific knowledge to the tillage of the soil. This is so obviously true that it would seem superfluous to urge the generous maintenance of the department in its grand work. Great as has been the work undertaken and accomplished, gratifying as have been the results, as shown in the first few pages of this report, be it remembered that we are still at the threshold of agricultural development, and that the educational work which has led to such grand results has only been extended as yet to a portion of our agricultural population. There is not an intelligent, patriotic citizen in the Union who will not say with his whole heart, 'Let the good work go on.'"

THE WILL OF ALFRED BEIT.

MR. ALFRED BEIT, who accumulated a vast fortune in South Africa and died on July 16, has by his will given large sums for public purposes. The most notable bequest is $6,000,000 to his partners to constitute a fund, the income of which is to be devoted to the construction, equipment or furtherance of any such methods of communication or transportation in Rhodesia, Portuguese Southeast Africa or the German possessions, and any parts of Africa that may be traversed by the Cape-to-Cairo Railway. The trustees are to have absolute discretion, and if two thirds decide that the fund is no longer required for furthering the work of communication or transportation, they can apply the proceeds to educational, charitable or other public purposes in Rhodesia.

One million dollars is left to the University of Johannesburg to build and equip buildings on the land previously given by Mr. Beit; one million dollars for educational or charitable purposes in Rhodesia and other territories within the field of the British South Africa Company; $125,000 to the Rhodes University, Grahamstown, Cape Colony; $100,000 for educational or charitable purposes in the Transvaal, and $75,000 for similar purposes in Kimberley and in Cape Colony. To the College of Technology, London University, the sum of $250,000 and 1,000 shares in the DeBeers Company are bequeathed, and to the research fund of London University $125,000. Two hundred thousand dollars is to be distributed equally in London and Hamburg for educational or charitable purposes. To King's Hospital and Guy's Hospital, London, the sum of $100,000 each is given. Mr. Beit's property

near Hamburg, which was his birthplace, is left to that city, and his art collections are left to the galleries in London, Berlin and Hamburg.

SCIENTIFIC ITEMS.

WE regret to record the death of Dr. Samuel Lewis Penfield, professor of mineralogy at the Sheffield Scientific School of Yale University, and of Dr. Paul Drude, professor of physics in the University of Berlin.

SIR DAVID GILL, H. M. astronomer at the Cape of Good Hope, has been elected to succeed Dr. E. Ray Lankester, director of the British Museum of Natural History, as president of the British Association for the Advancement of Science. The association will meet next year at Leicester, beginning on July 31. The meeting the following year will be in Dublin, and in 1909 the association will for the third time visit Canada and meet in Winnipeg.

A KNIGHTHOOD has been conferred on Dr. W. H. Perkin, F.R.S., the jubilee of whose discovery of the aniline dye mauve has recently been celebrated.—Professor Seubert, hitherto the German member of the international committee on atomic weights, has resigned, and Professor Ostwald has been appointed his successor. The committee now consists of F. W. Clarke, United States, chairman; T. E. Thorpe, Great Britain; H. Moissan, France, and W. Ostwald, Germany.—Mrs. W. P. Fleming, curator of astronomical photographs in the Harvard College Observatory, has been elected an honorary member of the Royal Astronomical Society. Mrs. Fleming has also been appointed honorary fellow in the department of astronomy in Wellesley College.

THE General Education Board, endowed by Mr. John D. Rockefeller with $10,000,000, has made appropriations to nine institutions on condition that three times the sum be appropriated from other sources. The appropriations, which amount to $312,500, are as follows: Coe College, Cedar Rapids, Ia., $50,000; Washburn College, Topeka, Kan., $25,000; Tulane University, New Orleans, $75,000; Wofford College, Spartanburg, S. C., $25,000; Furman University, Greenville, S. C., $25,000; Wake Forest College, Wake Forest, N. C., $37,500; Howard College, Birmingham, Ala., $25,000; Southwestern University, Jackson, Tenn., $25,000, and Mississippi College, Clinton, Miss., $25,000.